THURSDAY, AUGUST 18, 1892

A DEBATABLE LAND—PLANTS OR ANIMALS?

A Monograph of the Myxogastres. By George Massee. (London: Methuen and Co., 1892.)

THIS work is much in advance of any book in the English language treating of the perplexing group of organisms that forms its subject. The author has been in peculiarly favourable circumstances for the preparation of such a monograph, having enjoyed full access to "the splendid collection of Myxogastres in the Royal Herbarium, Kew, rich in types, and with numerous annotations by Rostafinski," as well as had "the loan or gift of valuable, and in some instances unique, specimens" from other workers in the same field in different countries. He has fully acquainted himself with the literature of the subject, and has made many personal observations on the structure, and, in some cases, on the life-history, of various types. He is thus able to bring to bear on the discussion of the problems that claim consideration a wide and varied knowledge; with the result that the book is indispensable to every student of the Myxogastres. The introductory portion will be found worth perusal by others besides specialists, as it discusses the arguments for and against the vegetable nature of the group. While accepting the view that the origin of the Myxogastres is to be found among the *Flagellatæ*, he comes to the conclusion that the sum of the characters presented by them in the reproductive phase manifests a tendency "in the direction of the vegetable kingdom, and more especially in the direction of the Fungi." But he is unable to establish strict homologies with the latter; since the Myxogastres are "a terminal group, and permit no comparisons with higher forms of the same type." In the discussion of this vexed question, Mr. Massee shows none of the virulence to which it has given rise in former times; and he endeavours to do justice in his statement to the views of De Bary, and of other supporters of the view that the organisms in question should be regarded as animals. After all, to an evolutionist at least, the distinction would appear more of verbal than of real importance.

Accepting the view that there are certain forms, of a very primitive structure, from which the animal and vegetable kingdoms have been developed in ever-increasing specialization, there is cause to expect the existence of more or less intermediate types, in which the characters at one period of their life-history are more those of animals, and at another period more plant-like. It matters little under which kingdom we agree to place them. Here, as elsewhere, Nature refuses to be bound down by rigid classification, and we must accept facts as they are, not as any *a priori* system might wish to make them. That the Myxogastres are not Fungi may be admitted; though they show a considerable similarity in various points in the course of development—a similarity clearly stated by Mr. Massee in his discussion of the whole question.

The term *Myxogastres* is employed, we infer, on the ground of priority. It possesses, however, the incidental advantage of not implying any positive view on the nature of the group, in the same way as do the terms *Myxomycetes* and *Mycetozoa*. The limits of the group are taken in this monograph in the sense employed by Rostafinski. It thus does not include the *Acrasieæ* and *Ceratium*, admitted as Mycetozoa by De Bary; nor does it make any reference to the numerous forms of the *Monadineæ*, which Zopf discusses in his "Schleimpilze" in Schenck's "Handbuch der Botanik." Thus limited, the group is more homogeneous; though the definition is, perhaps, some what arbitrary, and omits forms that are undoubtedly related to the more specialized types, and an English work on which would be welcome.

The author enters on somewhat slippery ground in the endeavour to explain the line of development of the Myxogastres, and also to illustrate his ideas of the relationship between the several orders. He holds that four orders can be distinguished by the presence or absence of lime in the sporangial wall, and by the presence and nature of the capillitium.

"In each order we find the special characteristic idea evolving through a sequence of genera, the terminal one not connected with any higher order, hence the special feature terminates abruptly within the order where it originated, and it is invariably in some comparatively undifferentiated genus near the initial point of each order, that we meet with the suggestion of a new line of evolution, which, at its maximum of development, constitutes the characteristic feature of the order immediately in advance of the one from which it emanated in an incipient condition."

Turning now to the systematic portion of the work, we find that it gives abundant proofs of care and of familiarity with the several forms, based on personal examination of each. The method of description is clear, the more important characters being printed in italics. Mr. Massee recognizes fully the difficulties of determining the limits of species and of the larger groups "while the life-history of the majority of forms is still unknown," saying plainly that

"all attempts at classification, as also the conception as to what constitutes a species, must be considered as tentative. When we are better acquainted with the main lines of development and lines of variation, also the conditions determining these variations, it is certain that the main factor in the discrimination of species will not be a one-twelfth oil-immersion objective."

Basing his acquaintance with the Myxogastres on personal examination of large numbers of examples, fresh and dried, many of the latter being authentic types, Mr. Massee does not hesitate to unite species and genera hitherto kept as distinct, but shown to be connected by fuller material. Thus several familiar names become sunk as synonyms; *e.g. Licea*, Schrad., and *Lindbladia*, Fr., are ranked under *Tubulina*, Pers. (emended). Under the generally accepted rules of nomenclature, this leads to Massee standing as the authority for many species, transferred by him, in reality, to another genus. But, besides such cases of apparent novelties, there are also a good many descriptions of *new* species in the usual sense of the term. The synonyms are carefully given under each group and species. A wise reticence has been observed in the endeavour to recognize the species meant by most of the older writers who mention the Myxogastres. The

synonymy previous to Rostafinski's monograph is borrowed as a whole from that work, "without any attempt at corroboration." Mr. Massee says:—

"I feel certain that nearly one-third of Rostafinski's work would not have been sacrificed to synonyms unless they mean something more than I have been able to discover, hence I have not felt justified in ignoring them altogether."

The geographical distribution has been worked out from the extensive collections already referred to as at the author's command.

The twelve plates, bearing 313 coloured figures by Mr. Massee himself, call for special mention as a valuable assistance to students of the Myxogastres. They deserve high praise for their accuracy and execution. The printing and get-up of the book are very satisfactory. A review would scarce be complete did it not find fault with some point or other; and we may do that part of our duty very briefly by taking exception to the rather inconvenient size (large octavo), and to the tendency in the introductory pages to let the sentences run to an inconvenient length. One, taken at random, we found to occupy twenty-five lines. There is no ground for this charge, however, as regards the descriptive portion of the monograph.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

The Apodidæ—a Reply.

PROF. LANKESTER'S review of my book in NATURE (p. 267) contains, as is usual with "candid opinions," a considerable number of misstatements. These compel me to ask space for a reply.

Prof. Lankester commences by stating very authoritatively that my account of the hermaphroditism of Apus is erroneous. This question, being purely a matter of evidence, can wait. My account of it in "The Apodidæ" is "meagre" because, as is clear to any one who reads the preface, I was constrained to put aside for the present all questions which did not directly bear upon the line of argument embodied in my book.

These points, however, are not serious. Let us turn, then, to the main charges which are intended to deprive my book of all claim to be a real contribution to zoological science. Prof. Lankester, after himself dethroning my title, "The Apodidæ," says that I "pose as the discoverer of a new and unsuspected agreement between the Crustacea and the Chætopoda, and that I bring forward arguments as new which have "long been effectively used" for the same purpose. It is difficult here not to accuse Prof. Lankester of deliberate misrepresentation. If he will allow me to keep my title and will read my book, he will find that I go beyond this general standpoint, and specialize the Apodidæ as the particular Phyllopods which are to be deduced from a Chætopod. Without, I believe, a single page of digression, my book discusses from beginning to end the relation of the Apodidæ to the Annelids, of the Apodidæ to Limulus, to the Trilobites, and so on. All the well-known arguments in favour of the more general proposition which deduces the Phyllopods from Annelids I have naturally adopted, adding, however, many new arguments of more or less weight in favour of my special point. Not one of these arguments does Prof. Lankester attempt to meet. The only one he refers to he wishes to claim as his own, as, indeed, he does everything else in the book "which will bear examination"! This charge of wholesale plagiarism from Prof. Lankester's articles on Apus and Limulus is the more remarkable, because my own investigations compelled me either to modify or to reject almost every position therein adopted by him. This may account for his "candid opinion," but hardly for his charge of plagiarism. The only evidence he adduces to support this charge can merely be meant to throw dust in the eyes; it is as follows:—

In describing the absence of articulations in the limbs of Apus I admitted that Prof. Lankester had noted the point (which, however, is not absolutely correct), but I added that he had failed to see its significance. Prof. Lankester resents this statement, and cites himself to show that he agreed with Claus in holding that the limbs of the Arthropoda were homologous with the parapodia of the Chætopods. This acquiescence in a general proposition does not in any way prove that he applied it to explain the special conditions of the limbs of Apus.

While I do not at all share his jealousy in matters of priority, and will gladly yield the point to him if he can base his claim on something more definite than the passage he cites, the fact that he wishes to claim this argument for his own is specially interesting. There is far more meaning in this than in his use of such expressions as "fanciful conceptions, crude speculations, and dogmatic assertions," because, if this particular argument holds—and Prof. Lankester would not claim it unless he acknowledged its validity—it goes far to show that my theory can hardly be called a "fanciful conception." The reviewer's statement that "there is no evidence" that I "made use of well-preserved material," looks as if he had not taken the trouble to read the book, and further as if he did not understand the importance of the issues at stake; the histological points, which are the only ones likely to be affected by the state of preservation of the material, are insignificant as compared with the main argument.

If, instead of indulging in such loose charges, Prof. Lankester had endeavoured to show where, in his opinion, my argument breaks down, and what are some of the more glaring misstatements in my book, which cause him to "regret" that he cannot recommend it as "a repository of fact," he would have done science (and perhaps (?) myself personally) much better service. I should also personally have been grateful to him had he himself set an example to the more "inexperienced" zoologist of "how morphological problems should be attacked." I did not, in my speculations as to the relation of Apus to the Annelids, feel inclined to follow the example set by Prof. Lankester in his own speculations as to the relations of Limulus to the Arachnids. I was especially recommended to ripen my ideas, and to publish them together in book-form. Would Prof. Lankester have advised me to publish my speculations, as he did his, in separate articles, occasionally, perhaps, advancing theories and arguments in one article which have to be withdrawn in the next? This plan may be convenient for the writer, but is most annoying to all who have to work over the same ground again.

To conclude, my book is an argument from beginning to end; the argument may be absurd, but it must be met by argument. In the meantime, until Prof. Lankester demolishes it, I have the good fortune to know that several leading zoologists, among whom Prof. Haeckel kindly permits me to mention his name, think it—well, to say the least—*not* absurd.

August 2. HENRY M. BERNARD.

Calculation of Trajectories of Elongated Projectiles.

(Additional Note.)

IT has been already pointed out (NATURE, March 1892, p. 474) that the range table of the 4-inch B.L. gun, selected by the authorities, afforded a more satisfactory test of the value of the coefficients of resistance than the results of the special experiments carried out with that gun in 1887. This range table was based on practice of 17/5/83, 7/3/84, and 21,23/4/84. The muzzle velocity was 1900f.s.; the weight of the shot 25lbs.; and the diameter of the shot 4in. But no information is given respecting the height of the barometer or thermometer. In this table the elevations are given at which the gun must be laid to obtain ranges of 100, 200, 300 .... 7600, 7700 yards, and also the time of flight for each range, expressed to the $\frac{1}{100}$th of a second for ranges below 5000 yards, and to the $\frac{1}{10}$th of a second for ranges 5000 to 7500 yards.

In calculating the ranges for elevations of 1°, 2°, 3° .... 20°, the temperature was supposed to be 62° F., and height of the barometer 30in., at the level of the gun. The coefficient κ was supposed to be 0·97 to adapt the tables to a head struck with a radius of two diameters.

By the use of the range table it was found what was the experimental elevation and time of flight for each of the ranges obtained by calculation.

The results of calculation and experiment are given in the following table. In column 1 the calculated ranges are specified. In columns 2 and 3 the calculated and experimental corresponding times of flight are given, and in column 4 the differences of these quantities. In columns 5 and 6 the calculated and the experimental elevations are given, and in column 7 their differences, which are due to the "jump" of the gun and to the "vertical drift" of the elongated shot. The calculated horizontal remaining velocity (column 8) is given in each case in yards per second to facilitate the expression of the small errors in time, given in column 4, in yards of range.

By the use of the general tables the time of flight over each range and the horizontal remaining velocity have been calculated (see columns 10 and 9), supposing the shot in each case to start with the horizontal muzzle velocity, and to move through air of a density corresponding to the mean height to which the shot actually rises.

Range.	Time of flight.			Elevation.			Calculated horizontal remaining velocity.	General tables	
	Calc.	By R. T.	Diff.	Calc.	By R. T.	Diff.		Rem. vel.	Time.
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Yards.	Secs.	Secs.	Secs.	°	° '	'	y. s.	y. s.	Secs.
1053	1·92	1·90	+0·02	1	0 52	+ 8	479	478	1·92
1831	3·72	3·77	−0·05	2	1 56	+ 4	391	390	3·73
2433	5·36	5·44	0·08	3	2 58	+ 2	343	345	5·38
2937	6·86	7·00	−0·14	4	3 56	+ 4	322	322	6·89
3386	8·30	8·41	−0·11	5	4 53	+ 7	305	304	8·33
3797	9·64	9·77	−0·13	6	5 51	+ 9	293	289	9·73
4148	10·95	11·04	−0·09	7	6 44	+16	278	278	10·98
4467	12·21	12·23	−0·02	8	7 35	+25	265	268	12·15
4813	13·46	13·57	−0·11	9	8 36	+24	258	258	13·48
5110	14·66	14·74	−0·08	10	9 33	+27	249	250	14·66
5384	15·84	15·84	0·00	11	10 28	+32	242	242	15·81
5664	17·01	16·95	+0·06	12	11 25	+35	234	235	16·98
5924	18·14	18·10	+0·04	13	12 20	+40	227	228	18·12
6170	19·29	19·18	+0·11	14	13 12	+48	220	222	19·24
6398	20·40	20·29	+0·11	15	14 5	+55	214	217	20·30
6632	21·53	21·46	+0·07	16	15 0	+60	208	212	21·41
6821	22·58	22·51	+0·07	17	15 46	+74	203	207	22·34
7003	23·62	23·52	+0·10	18	16 33	+87	198	203	23·26
7221	24·75	24·71	+0·04	19	17 33	+87	192	199	24·38
7483	25·94	26·10	−0·16	20	18 47	+73	188	193	25·75

The very small differences in column 4 between the calculated and experimental times of flight for the full extent of the range table afford conclusive evidence of the accuracy of the coefficients of resistance derived from my experiments of 1867, 1868, and 1878–80.

F. BASHFORTH.

A Plea for an International Zoological Record.

BEING now for the second year one of the Recorders for our English "Zoological Record," I should like to offer a few remarks upon the disadvantages of the system of recording that prevails at present in England and abroad.

The first point to be noticed is the number of independent Records that are published. Chief among these are our *Zoological Record* and the *Zoologischer Jahresbericht*, published by the Zoological Station at Naples. Besides these there are several minor semi-private records which it does not concern us to enumerate.

The disadvantage of so many records is obvious In the first place they are expensive, as the result of competition is to decrease the number of purchasers of each record, since comparatively few zoologists are able to purchase more than one of them. Secondly they are all in some way incomplete. Thus without going into great details it may be pointed out that the *Zoological Record* specializes upon systematic zoology, and as a result the portions devoted to animal morphology and embryology are all but useless, as a rule, to those interested in these subjects. Moreover, the systematic portion, being often undertaken by zoologists who are not professed systematists, does not appear always to give satisfaction to those it is intended to benefit. On the other hand the *Zoologischer Jahresbericht* leaves out systematic zoology entirely, which in many groups of animals cannot well be separated from other branches of study, and from the fact that it does not record palæontological papers, there are often omitted, at least in my two groups—sponges and echinoderms—many works of great morphological importance.

Some years ago a proposal was made by Dr. Dohrn and the staff of the Naples Zoological Station to unite the two records into one. The English part was to be entirely systematic, the Naples part was to be entirely morphological and physiological, and both were to be published together as parts of one record. This most excellent proposal was refused by the British zoologists, owing, apparently, to a desire to gain exclusive honour for British nationality.

I wish now to propose that this long-delayed project should be carried out, and that in future one International Zoological Record should be published. Such a record should fall into two natural parts: (1) a morphological and physiological part, and (2) a systematic part; each with its own chief editor. Seeing now that Naples is a recognized centre for zoological research, and that a modern zoologist's education is scarcely complete until he has studied there, the first part of the Record could best be done there much on the lines of the present *Jahresbericht*. On the other hand London, with the greatest systematic collection in the world and the addition of a perfect library, would naturally be the centre for the systematic part. The total result could be published in one volume, perhaps best at Leipzig, and the systematic part would be in English, while the morphological part could be in English, French, German, or whatever might be the language of the recorder, as it is now in the Naples *Jahresbericht*.

I think the advantages of such a scheme are obvious. By the combination the labour of recording would be enormously lessened, and the combined record need not be much more expensive than either one of the two now existing. At the same time authors could be encouraged to send in abstracts of their own works to one of the two editors. This would be an advantage in every way. In the first place authors would be sure of seeing a proper abstract of their papers published. I am sure it must be the experience of many who have published a mêmoir and afterwards read the abstract of it, that the abstract often gives a shockingly mutilated account of the results set forth in the original paper. By the omission of a qualifying phrase or sentence, an author's results are often made to appear in abstracts as absolute rubbish. I speak as one who has suffered.

On the other hand, the work of the recorder would be still further lightened by authors sending abstracts of their own works. It might be left to the editors' discretion to cut down an abstract if it was too long.

I feel confident that a scheme of combined records such as I have sketched out would cheapen the production of the Record to such an extent, that, amongst other things, it would be possible to pay a special recorder of the literature, that is to say, a person whose business it would be to go through all the periodicals and sort out the papers amongst the different recorders, as is actually done by the editor of the *Naples Jahresbericht*. In the present system of the English *Zoological Record* each recorder has to go through the whole of the periodicals, and if the group be a small one, *e.g.* sponges, the labour of searching for papers is out of proportion to the task of recording them. Moreover, it necessitates a longer or shorter residence in London near the British Museum Library, which may cost a recorder *more than he is paid* for his share of the Record. The duties of a recorder of the literature would be best undertaken by some one residing near the British Museum, as he could then get all the periodicals. During my residence in Naples last year I was unable to obtain all the periodicals in the enormous list of the *Zoological Record*, and thus I was obliged to leave out of my *Sponge and Echinoderm Record* for 1890 a great many papers which I am recording now for 1891.

A great need at the present moment is the intelligent organization of scientific research, and I venture to suggest the above

scheme as an improvement upon the present organization as far as the recording of zoological literature is concerned. Perhaps in the far distant future a record of geology and botany might further be incorporated in the above scheme, to make an "International Record of the Progress of Biological Science." It is scarcely to be hoped, however, that we are within a measurable distance of such a convenience. Would it not be a reasonable thing that the Royal Society of London should initiate such a progress in the recording of scientific literature as that here advocated?

E. A. MINCHIN.

University Museum, Oxford, August 2.

Pilchards and Blue Sharks.

YOU may like to know that the pilchards in coming in on the Cornish coasts this season are followed by great quantities of blue sharks (*Squalus glaucus*) from four to nine feet in length.

Just now they are hanging about four or five miles from land, and evidently are disturbing the pilchards in their feeding very much, as they are not scattering and playing on the surface of the sea in the evening twilight as they usually do, but are keeping in closely-packed schools throughout the night; hence our fishermen are having a very uncertain time of it as the consequence, some boats having rather heavy catches, and others only a few hundred of fish. And all are complaining of the damage done to the nets by the sharp teeth of these monsters, as in attacking the pilchards in the fishermen's nets, there is no hesitancy on the part of the sharks, for the net is bitten through and carried off with the pilchards. Last Friday morning the fishing-boat *Wave* landed seven of these sharks, and the master said, had he desired it, he could have caught a dozen, or more.

MATTHIAS DUNN.

Mevagissey, Cornwall, August 16.

Aurora Borealis.

STANDING by the Hampstead Heath flagstaff last Friday evening (12th), a few minutes before ten, I witnessed a feeble but characteristic display of the Aurora Borealis. Looking to the north-west, and midway between Ursa Major and the horizon, was a speck of pale bluish-green luminousness. While wondering as to the cause, a flickering shaft of crimson-tinted light shot upward in the direction of the "Pointers." This was followed by other streamers and "glows," sometimes white, sometimes slightly coloured. Occasionally patches of hazy light would be formed, through which the stars could be seen, and once a number of horizontal bands or waves passed upward from the horizon in quick succession, travelling almost to the star G in Ursa Major before they faded away. At 10.20 p.m., when I left the spot, the streamers had apparently ceased, but the sky was still luminous. Throughout the display was very faint and the colours very weak—mere tints.

A. BUTCHER.

ON Friday evening, August 12th, between the hours of nine and ten p.m., there was visible here a magnificent display of the aurora borealis. The streamers were very bright at times, and those on each extreme were more or less reddish. I think it worth recording because of the unusual time of year for such a display. It was doubtless seen over a wide region, and the telegraph system may have had some experience of earth currents.

EDMUND MCCLURE.

Mundesley, Norfolk, August 13.

AN active aurora of great brilliancy was visible here on Friday night from nine till ten p.m. The whole realm of the sky from north-west to north-east and from horizon to zenith was filled with a vaporous and highly luminous mass with streamers and rays, the light sufficing for reading moderately large type. The streamers and rays were projected from the upper edge of an arch of dark-coloured vapours resting on the northern horizon. The sky space occupied by the points of the streamers covered the constellation Ursa Major on the west, Cassiopea on the east, and the intermediate region. Among the brilliant sheaf of white streamers an occasional dark-coloured ray shot upwards from the generating arch.

J. LLOYD BOZWARD.

Worcester, August 13.

Aurora Australis.

A FRIEND (Mr. Hamilton S. Dove) who has resided for several years in Tasmania having sent me a full account of an unusually splendid aurora recently observed by him, but which met with little notice even locally, I enclose a condensed description of it, thinking it worthy of record in your columns. In previous occurrences of Southern Aurora he had observed "only a greenish yellow light, and was very much surprised at the grand fiery-red cloud-like patches, which formed so striking a feature in this phenomenon."

WILLIAM WHITE.

The Ruskin Museum, Sheffield.

"On the night of Wednesday, May 18th, 1892, a grand display of aurora was witnessed by us in the Calder district, near the township of Wynyard, Table Cape, North-west Tasmania. The sun had set at about ten minutes to five, and the night was very clear and cold—no clouds were to be seen—with a keen frosty wind blowing from the south-west. Shortly after seven o'clock a bright light was visible above the southern horizon, somewhat similar to the light preceding sunrise. Then two broad zones of greenish light appeared, extending from the south-east to the south-west, in the form of a depressed arch, one zone being a short distance above the other, like the bands of a rainbow. At times parts of these bands faded, whilst other parts became brighter. Presently some patches of a dark-red colour, as of illuminated sunset clouds, began to appear above the zones of greenish light, spreading along, but with intervals between, the whole expanse of the zones—one specially large and deep red patch being conspicuous in the extreme south-west. These patches glowed and faded alternately in the same manner as the zones of greenish light.

"After continuing for the space of about half an hour the coloured lights gradually faded, leaving the strong whitish light which appeared at first. Towards nine o'clock, however, a further manifestation occurred, beginning with a brilliant red light in the south-east, and extending from the horizon to a considerable distance upward, resembling the glow from a huge fire. This also paled and brightened, till presently the two broad zones of greenish light again appeared, this time, however, confined chiefly to the south and south-east heavens, very little reaching south-west. After this reappearance of the zones some broad white stripes commenced to radiate from the horizon, crossing the zones more than half the way upwards to the zenith. The stripes began to appear near the red glow in the south-east, and several others occurred south-east by south, only two rather faint ones being to the west of south.

"Almost directly one of the white stripes appeared one of the red cloud-like patches came to the east of it, and gradually extended towards it, so that the sky above the zones of greenish-yellow light was eventually covered with red glowing patches and pale vertical stripes, which similarly paled and brightened.

"The later appearances, like the first, lasted for about half an hour and then disappeared, the moon rising soon afterwards.

"H. S. DOVE.
"G. W. EASTON."

Units Discussion at British Association.

REFERRING to the preliminary memorandum printed in your issue of August 4th, page 334, I wish to correct a slip in the statement about the fall between two surfaces joined by a "weber." I ought to have added, "if their area is one square centimetre." Enlargement of the area to a metre would diminish the pull to 40 tons. Also I may observe that at the meeting I did not press all the proposed resolutions, but withdrew Nos. 4, 5, 6, and 8.

OLIVER J. LODGE.

THE VARLEY TESTIMONIAL.

AN important Committee, containing among others, Lord Kelvin, Prof. Ayrton, Prof. G. Forbes, Dr. Gladstone, Prof. D. E. Hughes, Dr. J. Hopkinson, Prof. Kennedy, Prof. O. Lodge, Prof. J. Perry, Messrs. W. H. Preece, A. Siemens, A. Stroh, J. W. Swan, and Prof. S. Thompson, has been formed, to give effect to the feeling amongst some of the older members of the electrical profession that the life-long labours in electrical research of Mr. S. A. Varley should be recognised by some substantial testimonial befitting his reputation as a scientific investigator.

A brief sketch of Mr. Varley's career will serve to show what signal services he has rendered to the cause of electrical science and the honour his discoveries have conferred upon this country.

Mr. Samuel Alfred Varley was born in London in 1832, and was the third son of the late Mr. Cornelius Varley, an active man of science and an artist. In 1858, when the Atlantic cable was being constructed, he wrote a paper, read before the Institute of Civil Engineers, "On the Electrical Qualifications requisite in Long Submarine Cables," and was shortly afterwards elected an Associate Member of that Institution.

In the paper referred to above, Mr. Varley opposed the views of the electrical advisers of the company. Faraday, who had publicly supported their opinions, endorsed Mr. Varley's ideas immediately after receiving a copy of his paper. Mr. Varley followed this up by reading a second one before the Society of Arts in 1859, "On the Practical Bearing of the Theory of Electricity to Long Submarine Telegraphy." In this paper he suggested, among other things, the use of artificial lines, which have since proved of such value in connection with duplex working. In 1866 Mr. Varley discovered for himself the re-action or self-exciting principle, and at that early date constructed his first machine of the pure dynamo type, which is now in the Museum at South Kensington. His dynamo of 1866 was exhibited at the Inventions Exhibition of 1885, and for this he was awarded a gold medal.

The controversy which subsequently arose on this invention may be held to have been fitly summed up by the late Robert Sabine, C.E. (son-in-law of Sir Charles Wheatstone), in the following words:—"Professor Wheatstone says he was the first to complete and try the reaction machine. Mr. S. A. Varley was the first to put the machine officially on record in a provisional specification, dated December 24, 1866, which was, therefore, not published until July, 1867. Dr. Werner Siemens was the first to call public attention to the machine in a paper read before the Berlin Academy on the 17th January, 1867." (See *Engineering*, November, 1877.)

In 1866 he introduced needle-telegraph coils, in which soft-iron magnetically-induced needles were substituted for tempered steel needles. These induced and consequently undemagnetisable needles entirely superseded the old form introduced by Wheatstone and Cooke, and were largely adopted by the Postal Department. In the same year (1866) he designed a system of electric train inter-communication.

In the year 1875 Mr. Varley became assistant-manager of the works of the late British Telegraph Manufactory, Limited, and as the first Gramme machines constructed in England were manufactured by this firm, he had ample opportunities of studying the characteristics of both series wound machines and those having a seperate armature for excitation of the field magnets. There is scarcely a doubt that Mr. Varley's investigations at this period led to the invention of compound winding, for in 1876 he patented a series-shunt or compound-wound dynamo, and, in three legal suits, the claim that this specification first described a system of compound winding has been fully sustained. Mr. Varley has from time to time contributed papers read at the meetings of the British Association, among which may be mentioned one "On the Mode of Action of Lightning on Telegraph Circuits," wherein he described a lightning bridge designed by himself, a number of which are now doing duty, although fitted up more than twenty years ago.

But Mr. Varley's *magnum opus* is the important part which he took in the invention and perfecting of the dynamo, perhaps the most striking invention of the century, and upon this his fame as a patient, conscientious, and earnest scientific investigator of the Faraday school will permanently rest. His researches were undertaken in the true spirit of science, and no thought of self-emolument has ever caused him to deviate from the path which he has pursued throughout an eventful, although eminently simple and blameless life, a life in which self-denial and self-sacrifice have had no small share. Like many men of genius he was far ahead of the times, and has lived to see others reap the benefit of his great discoveries. His nervous and retiring disposition has for years kept him from the busy haunts of men, and to the younger generation of electricians he exists only in name, a name, however, that will live as long as the dynamo is employed in the service of man.

Subscriptions will be gladly received by the hon. treasurer, Mr. Stroh, 8 Haverstock Hill.

NOTES.

THE *Electrician* for August 5 contains an article on Lord Rayleigh, which is accompanied by a steel portrait.

AT a recent meeting of the Berlin Geographical Society, the chairman, Baron von Richthofen, announced that the society was about to publish, in commemoration of the 400th anniversary of the discovery of America, a work descriptive of the ancient manuscripts and maps in the Italian libraries relating to the history of this event. The German Emperor has promised a contribution of 15,000 marks towards the expense of the undertaking, and it is to be edited by Dr. Kretschmer. The accompanying atlas will contain thirty-five large maps, of which thirty-one are new, and will be published for the first time.

AT the lunch in the Library Hall, St. Andrews, on the 11th, to the party from the British Association, Prof. McIntosh announced that Mr. Charles Henry Gatty, of East Grinstead, had presented £1,000 for the purpose of establishing a Marine Laboratory at St. Andrews, which sum he further increased to £2,000 before the close of the day. The name of Mr. Gatty is sufficiently familiar to marine zoologists, were it only in connection with the accomplished lady (Mrs. Alfred Gatty), the favourite correspondent of Dr. George Johnston, of Berwick-on-Tweed. Mr. Gatty's munificent donation will enable St. Andrews to have a substantial and comfortable laboratory instead of the wooden building (formerly a fever hospital), which has hitherto been used for marine work since the period of the Trawling Commission under Lord Dalhousie. St. Andrews Marine Laboratory is the oldest permanent station in the country, and, as it has pre-eminent advantages in regard to varied and very rich marine fauna and flora, easy access to a fine University Library, and a University Museum—unique in certain departments, a new future is opened to it through Mr. Gatty's handsome gift. At the same meeting it was stated that the Fisheries prize of £20 given annually to the best student of Zoology (hitherto from an anonymous donor) was the gift of Mr. J. W. Woodall, of Scarborough. Both Mr. Gatty and Mr. Woodall were present.

DURING the past week the weather has been fine generally over the southern portion of the kingdom, but somewhat unsettled. The anticyclonic conditions which prevailed for a day or two

in the middle of last week gave place on Friday, the 12th inst., to a south-westerly current, with showery weather, the rainfall being rather heavy in the north and west, while the low pressure over the north of Scotland caused rather strong gales and heavy seas on Saturday and Sunday. During the early part of the present week a depression advanced from the southward, occasioning unsettled weather and fog or mist in places, while exceptionally heavy rains occurred in parts of Ireland and Scotland, the amount measured at Parsonstown on Monday morning being 1·24 inches. During the week the maximum temperatures have nearly reached 80° in some parts of England and in the east of Scotland. For the week ending the 13th inst., temperature was below the mean in all districts, except the Channel Islands, where it just equalled it. The absolute minima, which were registered on the 11th, were exceedingly low for the time of year, and at many of the more inland stations frost was experienced on the grass.

THE fifth annual report of the National Association for the Promotion of Technical and Secondary Education has just been published, and satisfactory progress is shown. During the year a bi-monthly journal has been issued under the title of the *Record of Technical and Secondary Education*, in which detailed accounts of the work done by the County Councils have been given from time to time. The *Record* has to some extent relieved the pressure on the space of the report, which is shorter than its predecessors.

THE *Ceylon Observer* for July 21 has an editorial on "Marine Biological Stations," and while sympathizing with the decision of the meeting which was called together a short time ago to take steps to establish a station in the island of Jamaica, ventures the hope that Ceylon, too, may have its marine biological station, and points out how particularly well situated that island is for such an undertaking.

WE refer elsewhere to Prof. Forel's report on the present extension of the Alpine glaciers, to which, whatever the *modus operandi*, the disaster of St. Gervais was due. A letter in Tuesday's *Times* refers to Mr. Douglas Freshfield's warning that there may still be an excess of water ready to discharge itself in the neighbourhood of the Aiguille du Gouté, and states that the view has received a remarkable confirmation. While a party was breakfasting at the Pierre Pointue, which overhangs the stream that drains the eastern wing of the Glacier des Boisons, a tremendous noise suddenly brought them all—visitors and *employés* of the chalet—out upon the platform to see the violent flood of opaque brown fluid which tore down the bed of the stream which had been flowing so quietly before. No great damage seems to have been done, but certainly the occurrence will strengthen the view that careful scientific studies should be made so that complete warning may in all cases be given.

AN address on "Geological Chronology," which Prof. Young delivered to the Physical Society of Glasgow University in February last, has been published in pamphlet form by Messrs. Carter and Pratt, Glasgow.

A PRELIMINARY draft prospectus of a new physical atlas, which Messrs. J. Bartholomew and Co., Edinburgh, have in preparation, has reached us. The work will be based upon Berghaus's "Physikalischer Atlas," published by Justus Perthes, of Gotha, 1889–92, but will, we understand, be much larger and more extensive, and contain a great deal of entirely new and original matter. According to the present intentions of the compilers, the work will be issued in five separate sections as follows: (1) Geology; (2) Orography and Hydrography; (3) Meteorology and Magnetism; (4) Botany and Zoology; (5) Ethnography and Geographical Demography, and when complete may be obtained either in one complete volume, or in five smaller volumes. The joint authors will be Mr. J. G. Bartholomew and Dr. H. R. Mill, and the various sections will be revised and edited by, amongst others, Prof. Bayley Balfour, Dr. A. Buchan, Sir Archibald Geikie, Prof. James Geikie, and Dr. John Murray.

THE *Times* of Tuesday states that Lieutenant Bower has discovered in Chinese Turkestan the remains of a subterranean city, in one of the excavations near which he found a curious birch-bark manuscript, which he took with him back to India for the investigation of scholars. The manuscript is described as having been dug out of the foot of one of the curious old erections just outside a subterranean city near Kuchar. These erections are said to be about 50 feet to 60 feet high, in shape like a huge cottage loaf, built solid with sun-dried bricks, with layers of beams now crumbling away. Dr. Hoernle, who undertook the examination of the manuscript, thinks that these erections are Buddhist stupas, which often contain a chamber enclosing relics and other objects. These chambers are generally near the level of the ground, and are often excavated by persons in search of hidden treasure. There is no reason why a birch-bark manuscript, thus preserved from the chances of injury, should not last for an almost indefinite period, especially if the chamber is air-tight. Dr. Hoernle has now communicated to the Asiatic Society of Bengal the result of his examination of the manuscript. It is written in Sanscrit of a very archaic type, not in the Sarada character of Cashmere, as was at first surmised, but in the Gupta character, which is a much earlier form. Separate portions of it were written by different scribes and at different dates, and the latest portion must, he thinks, be ascribed to a period not later than the second half of the fifth century—say 475 A.D.—while the earlier portion must be referred to a date half a century earlier. The manuscript is therefore the oldest Indian manuscript, and one of the oldest manuscripts existing in the world. The manuscript consists of fifty-five leaves, all of which have now been transcribed and the greater part translated by Dr. Hoernle, and both will be published in instalments by the Asiatic Society of Bengal.

THE *Times* India correspondent gives us some important intelligence regarding Mr. Conway's exploring party in the Hindu-Kush. The party has arrived at Askoleya after making the first definitely recorded passage of the Hispar Pass—the longest glacier pass in the world. The party left Nagar on June 27, spent ten days exploring the vast system of glaciers not marked on any map which covers the north slopes of the main Hindu-Kush range in that neighbourhood. Mr. Conway ascended a difficult rock peak of 17,000 feet, and attempted the ascent of the Great Nagar Mountain, but he was driven back by a hundred yards of ice fall, that proved to be absolutely impassable. On July 11, after a day's halt at Hispar, Mr. Conway started up the great glacier and reached the foot of Nushik in three short marches. The next day being cloudy he did not go to the top of Nushik, as he had intended, but he sent a party under Mr. Rondebush to cross that pass. They took all the spare baggage and conveyed it by the Braldo Valley to Askoleya. Meanwhile Mr. Conway and Mr. M'Cormick with an Alpine guide, Zurbuggen, continued three more marches up the great Hispar glacier to the pass, which they actually crossed on July 18. The view from the pass is said to be superb, over a vast lake of snow some 300 miles in area, quite flat, surrounded by a ring of giant peaks and with a row of peaks rising like islands in the midst of it. They camped just below the pass on the east side and were overtaken by a severe snowstorm. They descended two marches down the Biafo glacier to the level of grass, whence they sent Zurbuggen to Askoleya, which he reached in one long day's march. Mr. Conway spent six days on the way, chiefly occupied in surveying, which the continued bad weather rendered difficult. The whole party re-united at Askoleya on July 26. The length of

the pass from the foot of the Hispar Pass to the foot of the Biafo glacier is about ninety miles. The mercury on the pass stood at 15·85 in. No one suffered perceptibly from the rarefaction of the air.

THE latest news from Etna is that Monte Gemmellaro has broken out afresh, and the great lava current that has been flowing from it has now been divided into two arms, both of which are rapidly advancing in the direction of Serra Pizzuto and Pedova, completely covering *en route* the lavas of 1886. The deluges of molten rock that have been emitted during the last month have destroyed one of the finest and most fertile districts in Sicily.

PROF. FOREL has recently prepared a table (*Arch. de Sci.* July 15) showing the behaviour of the small lake at the Great St. Bernard in regard to cold since 1817. This lake is at a height of about 8000 feet, is about 24 acres in surface, and of small depth. It appears the mean duration of the frozen state is about 268 days; for nearly two-thirds of the year the lake is imprisoned "under a carapace of ice and snow." This justifies only too well the remark of the monk, "Nine months of winter and three months of bad weather." Between the earliest date of freezing (September 30) and the latest (November 6) are 36 days, the mean date being October 20. The earliest date of thawing is June 12, and the latest September 15; difference 95 days (the mean date being July 13). By grouping the dates in a series of decades (approximately) Prof. Forel finds maxima of duration of the frozen state in 1840–49 and in 1880–91, and a minimum in 1860–69. This corresponds fairly, he points out, with the phases of Brückner's cycle, according to which a maximum of cold occurred about 1850, a maximum of heat about 1860, and a maximum of cold about 1880.

BY the election of the present holder—Mr. A. A. Kanthack—to the Medical Tutorship of the Liverpool University College Medical School, the John Lucas Walker Studentship in Pathology at Cambridge will shortly become vacant. The studentship is of the annual value of £250, and is tenable for three years. Candidates should send in their applications and testimonials by October 25, to Prof. Roy, F.R.S., New Museum, Cambridge.

WE learn from the *British Medical Journal* that the Library of the British Medical Association has been presented with a valuable gift of a series of important works, bearing chiefly on hygiene and public medicine, from the library of the late Dr. Alfred Carpenter. The books number upwards of 250 volumes, and are the gift of Mrs. Carpenter.

AN interesting account of a visit by Mr. E. Satow to the ruins of Sukkhothai and Sawankhalôk, Siam, appears in the Journal of the Society of Arts, for August 12.

A NEW edition—the sixth—of "The Electric Light popularly explained," by A. Bromley Holmes, has just been brought out by Messrs. Bemrose and Sons, Limited.

"PAPERS and Proceedings of the Royal Society of Tasmania for 1891" has just reached us, and we learn from the report that during the year six meetings were held and thirteen papers were read; the income amounted to £393, and the expenditure to £235 1*s.* 1*d.*

A CATALOGUE of Standard English and Foreign Books on Chemistry and the Allied Sciences has just been issued by Mr. W. F. Clay, Edinburgh.

THE Annual Report of the Superintendent, Mr. J. H. Hart, on the Royal Botanic Gardens, Trinidad, for 1891, has lately been published, and much good work seems to have been accomplished during the year.

WE have received from the Australian Museum, Sydney, Parts I. (Cephalopoda) and II. (Pteropoda) of the Catalogue of the Marine Shells of Australia and Tasmania, which Mr. John Brazier is compiling.

A PAMPHLET on "The Dairy and its Equipment, with Practical Management of Milk and Cream," which has been written by Dr. H. J. Webb (the Principal of the Aspatria Agricultural College) in conjunction with Mrs. Edward Moul, and recently issued, is full of information likely to be of use to those engaged in dairy-work.

IN the *American Naturalist* for August, Dr. S. Lockwood gives a geological reason "Why the Mocking Birds Left New Jersey," and the report of Prof. Osborn's lectures on "Heredity and the Germ-Cells" is continued.

Naturae Novitates for July has reached us from the publishers, R. Friedländer & Sohn, Berlin.

THREE new volumes have been added to the excellent series entitled "Encyclopédie Scientifique des Aide-Mémoire" (Gauthier-Villars)—"Notions de Chimie Agricole," by J. Schlœsing, Fils; "Les Divers Types de Moteurs à Vapeur," by E. Sauvage; "La Bière," by L. Lindet.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. H. D. Bowditch; a Puma (*Felis concolor*), a Tayra (*Galictis barbara*) from Brazil, presented by Mr. J. E. Wolfe; three Martineta Tinamous (*Colodromus elegans*) from Bahia Blanca, Argentina, presented by Mr. F. W. Horn; a Slender-billed Cockatoo (*Licmetis tenuirostris*) from Australia, presented by Dr. J. G. Victor Sapp; a Californian Sea Lion (*Otaria stelleri*) from the North Pacific, an Indian Chevrotain (*Tragulus meminna*, ♀) from India, deposited; an Indian Oriole (*Oriolus kundoo*), two —— Himalayan Tree Pies (*Dendrocitta himalayensis*) from India, five Black-necked Tanagers (*Euphonia nigricollis*), six Thick-billed Tanagers (*Euphonia laniirostris*), a Violet Tanager (*Euphonia violacea*), a Greenish Tanager (*Euphonia chlorotica*), a Lead-coloured Tanager (*Hypophæa chalybea*) from Brazil, purchased; a Magellanic Goose (*Bernicla magellanica*) bred in Holland, six Himalayan Tree Pies (*Dendrocitta himalayensis*) from India, received in exchange.

OUR ASTRONOMICAL COLUMN.

THE PERSEIDS.—Mr. Denning, in the August number of *The Observatory*, remarks, with reference to the radiant point of the Perseids, that "the agreement of observation and theory is not perfect, especially as regards the shower at its earlier stages and at its termination, when the displacement appears to be somewhat greater than theory requires." From observations made on August 16, 1877, the radiant he deduced was $60^\circ + 59^\circ$, the theoretical position, according to Dr. Kleiber, being $54^\circ + 59^\circ$. In 1886, on the same date, from the path of a very bright Perseid, Mr. Denning obtained a radiant point of $53^\circ + 59^\circ$, a value closely approaching the theoretical one. As Mr. Denning has reason to believe that the Perseids will continue to fall until the 22nd of this month, and as the suspected radiant for this date is about $77^\circ + 56\frac{1}{2}^\circ$, our readers will have an opportunity of either verifying this position or obtaining one more accurate.

THE observations of the August meteors seem to have been sadly interfered with by the weather. So far we have received no communication respecting them.

"HIMMEL UND ERDE."—The August number of *Himmel und Erde* contains much that will be read with interest. Herr J. Plassmann contributes an article, which will be continued in the next issue, on variable and new stars, in which, after referring to the peculiar features of the late nova, he discusses the light curves of many well-known variables. A supplement to the note on the great February spot includes an illustration

showing the movement in the line of sight of the F line caused by the presence of a protuberance in the region of the spot. "The Length of our Earth-days," "A Lost Comet," and "Paris Scientific Undertakings" are titles of other communications, the last of which is a brief general survey of recent additions to our knowledge about the construction and movement of the visible universe.

ASTRONOMY AT THE COLUMBIAN EXPOSITION.—Arrangements are about to be made for organizing a series of congresses or conventions to be held next year during the progress of the World's Exposition. The preliminary address of the General Committee on Mathematics and Astronomy points out that such a congress should take advantage of the presence of the leading scholars of the world for the mutual interchange of ideas by presenting and considering investigations in special lines of research.

The sections dealing with Astronomy and Astro-Physics are eight in number and are as follows:—

Astronomy.

a. History of Astronomy.
b. Astronomical Instruments.
c. Methods of Observation.
d. Physical Astronomy.
e. Observatory Buildings.

Astro-Physics.

a. Spectrum Analysis.
b. Astronomical Photography.
c. Stellar Photometry.

The address further states that advice and suggestions with regard to the general conduct of the convention are earnestly invited, while special stress is laid on the scientific questions for future discussion. The Chairmen of the Special Committees of the several subjects under the charge of the General Committee are as follows:—

Pure Mathematics.—Prof. E. H. Moore, Chicago University.

Astronomy.—Prof. G. W. Hough, Dearborn Observatory, North-western University, Evanston.

Astro-Physics.—Prof. George E. Hale, Kenwood Astro-Physical Observatory, Chicago.

Among the names in the partial lists of the Advisory Councils on these two subjects we notice those of Prof. A. Cayley and Prof. J. J. Sylvester for mathematics, and S. Copeland (Astronomer Royal for Scotland), Prof. R. S. Ball, Prof. Gill, Mr. Norman Lockyer, the Earl of Rosse, Prof. Liveing, Prof. Dewar, and Dr. Huggins.

LUNAR ECLIPSE, MAY 11, 1892.—With reference to the lunar eclipse that occurred last May, *Astronomische Nachrichten* No. 3106 contains a series of accounts, which include the times of immersion and emersion of the objects on the moon's surface, gathered from the following observatories:—Bonn, Heidelberg, Breslau, Christiania, Prag, Kiel, and Kalocsa.

NUMERATION OF ASTEROIDS.—The *Astronomical Journal* No. 271 contains the announcement that an arrangement had been agreed upon by which the numeration of asteroids will in future be put on a sound basis. For the present Prof. Krueger will assign to these bodies the notation 1892 A, B, C...in the order in which their announcement is sent to the Telegraphische Central-Stelle, Prof. Tietzen, Director of the Rechen Institute in Berlin, in the meantime undertaking their definite numeration. This arrangement will be found to avoid all such confusion as has been experienced with regard to those asteroids about which sufficient information is not available for their orbital determinations. Although they will not now receive their numbers, they can easily be recognized by their lettering in the annual series.

THE BRITISH ASSOCIATION.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY PROF. C. LAPWORTH, LL.D., F.R.S., F.G.S., PRESIDENT OF THE SECTION.

IT has, I believe, been the rule for the man who has been honoured by election to the chair of President of the Geological Section of the British Association to address its members upon the recent advances made in that branch of geology in which he has himself been most immediately interested. It is not my intention upon the present occasion to depart from this time-honoured custom; for it has both the merit of simplicity and the advantage of utility to recommend it. In this way each branch of our science, as it becomes in turn represented, not only submits to the workers in other departments a report of its own progress, but presents by implication a broad sketch of the entire geological landscape, seen through the coloured glasses, it may be, of divisional prejudice, but at any rate instructive and corrective to the workers in other departments, as being taken from what is to them a novel and an unfamiliar point of view.

Now every tyro in geology is well aware of the fact that the very backbone of geological science is constituted by what is known as stratigraphical geology, or the study of the geological formations. These formations, stratified and unstratified, build up all that part of the visible earth-crust which is accessible to the investigator. Their outcropping edges constitute the visible exterior of our globe, the surface of which forms the physical geography of the present day, and their internal characters and inter-relationships afford us our only clues to the physical geographies of bygone ages. Within them lies enshrined all that we may ever hope to discover of the history and the development of the habitable world of the past.

These formations are to the stratigraphical geologist what species are to the biologist, or what the heavenly bodies are to the astronomer. It was the discovery of these formations which first elevated geology to the rank of a science. In the working out of their characters, their relationships, their development, and their origin, geology finds its means, its aims, and its justification. Whatever fresh material our science may yield to man's full conception of nature, organic and inorganic, must of necessity be grouped around these special and peculiar objects of its contemplation.

When the great Werner first taught that our earth-crust was made up of superimposed rock-sheets or formations arranged in determinable order, the value of his conclusions from an economic point of view soon led to their enthusiastic and careful study; and his crude theory of their successive precipitation from a universal chaotic ocean disarmed the suspicions of the many until the facts themselves had gained such a wide acceptance that denial was no longer possible. But when the greater Hutton asserted that each of these rock formations was in reality nothing more nor less than the recemented ruins of an earlier world, the prejudices of mankind at large were loosed at a single stroke. Like Galileo's assertion of the movement of the globe, this demanded such an apparently undignified and improbable mode of creation that there is no wonder that, even down to the present day, there still exist some to whom this is a hard saying, to be taken, if taken at all, in homœopathic doses and with undisguised reluctance.

Hutton, as regards his philosophy, was, as we know, far in advance of his time. With all the boldness of conviction he unflinchingly followed out these ideas to their legitimate results. He claimed that as the stratified formations were composed of similar materials—sands, clays, limestones, and muds—to those now being laid down in the seas around our present coasts, they must, like them, have been the products of ordinary natural agencies—of rain, rivers, and sea waters, internal heat and external cold—acting precisely as they act now. And further as these formations lie one below the other, in apparently endless downward succession, and all are formed more or less of these fragmentary materials, so the present order of natural phenomena must have existed for untold ages. Indeed, to the commencement of this order he frankly admits, "I see no trace of a beginning or sign of an end."

The history of the slow acceptance of Hutton's doctrines, even among geologists, is, of course, perfectly familiar to us all. William Smith reduced the disputed formations to order, and showed that not only was each composed of the ruins of a vanished land, but that each contained in its fossils the proof that it was deposited in a vanished sea inhabited by special life creation. Cuvier followed, and placed it beyond question that the fossilized relics of these departed beings were such as made it absolutely unquestionable that these creatures might well have inhabited the earth at the present day. Lyell completed the cycle by demonstrating stage by stage the efficiency of present natural agencies to do all the work required for the degradation and rebuilding of the formations. Since his day the students of

stratigraphical geology have universally acknowledged that in the study of present geographical causes lies the key to the geological formations and the inorganic world of the past.

In this way the road was paved for Darwin and the doctrine of descent. The aid which had been so ungrudgingly afforded by biology to geology was repaid by one of the noblest presents ever made by one science to another. For the purposes of geology, the science of biology had practically completed a double demonstration: first, that the extinct life discernible in the geological formations was linked inseparably with the organic life of the present; and, second, that every fossil recognized by the geologist was the relic of a creature that might well have existed upon the surface of the earth at the present time. Geology repaid its obligation to biology by the still greater twofold demonstration: first, that in the economy of nature the most insignificant causes are competent to the grandest effects, if only a sufficiency of time be granted them; and, second, that in the geological formations we have the evidences of the actual existence of those mighty eons in which such work might be done.

The doctrine of organic evolution would always have remained a metaphysical dream had geology not given the time in which the evolution could be accomplished. The ability of present causes to bring about slow and cumulative changes in the species is, to all intents and purposes, a biological application of Hutton's ideas with respect to the original geological formations. Darwin was a biological evolutionist, because he was first an uniformitarian geologist. Biology is pre-eminent to-day among the natural sciences, because its younger sister, Geology, gave it the means.

But the inevitable consequence of the work of Darwin and his colleagues was that the centre of gravity, so to speak, of popular regard and public controversy was suddenly shifted from stratigraphical geology to biology. Since that day stratigraphical geology, to its great comfort and advantage, has gone quietly on its way unchallenged, and all its more recent results have, at least by the majority of the wonder-loving public, been practically ignored.

Indeed, to the outside observer it would seem as if stratigraphical geology for the last thirty years had been practically at a standstill. The startling discoveries and speculations of the brilliant stratigraphists of the end of the last century and first half of the present forced the geology of their day into the very front rank of the natural sciences, and made it perhaps the most conspicuous of them all in the eyes of the world at large. Since that time, however, their successors have been mainly occupied in completing the work of the great pioneers. The stratigraphical geologists themselves have been almost wholly occupied in laying down upon our maps the superficial outlines of the great formations, and working out their inter-relationships and subdivisions. At the present day the young stratigraphical student soon learns that all the limits of our great formations have been laid down with accuracy and clearness, and finds but little to add to the accepted nomenclature of the time.

Our palæontologists also have equally busied themselves in working out the rich store of the organic remains of the geological formations, and the youthful investigator soon discovers that almost every fossil he is able to detect in the field has already been named, figured, and described, and its place in the geological record more or less accurately fixed.

In France, in Germany, in Norway, Sweden, and elsewhere, in Canada and in the United States, work as thorough and as satisfactory has been accomplished, and the local development of the great stratified formations and their fossils laid down with detail and clearness.

Many an unfledged, but aspiring geologist, alive to these facts, and contrasting the well-mapped ground of the present time with the virgin lands of the days of the great pioneers, finds it hard to stifle a feeling of keen regret that there are nowadays no new geological worlds to conquer, no new systems to discover and name, and no strange and unexpected faunas to unearth and bring forth to the astonished light of day. The youth of stratigraphical geology, with all its wonder and freshness, seems to have departed, and all that remains is to accept, to commemorate, and to round off the glorious victories of the dead heroes of our science.

But to the patient stratigraphical veteran, who has kept his eyes open to discoveries new and old, this lull in the war of geological controversy presents itself rather as a grateful breathing time; the more grateful as he sees looming rapidly up in front the vague outlines of those oncoming problems which it will be the duty and the joy of the rising race of young geologists to grapple with and to conquer, as their fathers met and vanquished the problems of the past. He knows perfectly well that Geology is yet in her merest youth, and that to justify even her very existence there can be no rest until the whole earth-crust and all its phenomena, past, present, and to come, have been subjected to the domain of human thought and comprehension. There can be no more finality in Geology than in any other science; the discovery of to-day is merely the stepping-stone to the discovery of to-morrow; the living theory of to-morrow is nourished by the relics of its parent theory of to-day.

Now if we ask what are these formations which constitute the objects of study of the stratigraphical geologist, I am afraid that, as in the case of the species of the biologist, no two authorities would agree in framing precisely the same definition. The original use of the term *formation* was of necessity lithological, and even now the name is most naturally applied to any great sheet of rock which forms a component member of the earth-crust; whether the term be used specifically for a thin homogeneous sheet of rock like the Stonesfield slate, ranging over a few square miles; or generically, for a compound sheet of rock, like the Old Red Sandstone, many thousands of feet in thickness, but whose collective lithological characteristics give it an individuality recognizable over the breadth of an entire continent.

When Werner originally discovered that the "formations" of Saxony followed each other in a certain recognizable order, a second characteristic of a formation became superposed upon the original lithological conception—namely, that of determinate "relative position." And when William Smith proved that each of the formations of the English Midlands was distinguished by an assemblage of organic remains peculiar to itself, there became added yet a third criterion—that of the possession of "characteristic fossils."

But these later superposed conceptions of time-succession and life-type are far better expressed by dividing the geological formations into zoological *zones*, on the one hand, and grouping them together, on the other hand, into chronological *systems*. For in the experience of every geologist he finds his mind instinctively harking back to the bare lithological application of the word "formation," and I do not see that any real advantage is gained by departing from the primitive use of the term.

A *zone*, which may be regarded as the *unit* of *palæontological succession*, is marked by the presence of a special fossil, and may include one or many subordinate formations. A *system*, which is, broadly speaking, the *unit of geological succession*, includes many "zones," and often, but not always, many "formations." A *formation*, which is the *unit of geological stratigraphy*, is a rock sheet composed of many strata possessing common lithological characters. The formation may be simple, like the Chalk, or compound, like the New Red Sandstone; but, simple or compound, local or regional, it must be always recognizable, geographically and geologically, as a lithological individual.

As regards the natural grouping of these lithological individuals as such, fair progress has been made of late years, and our information is growing apace. We know that there are at any rate three main groups: 1st. The stratified formations due to the action of moving water above the earth-crust; 2nd. The igneous formations which are derived from below the earth-crust; 3rd. The metamorphic formations which have undergone change within the earth-crust itself. We know also that of these three the only group which has hitherto proved itself available for the purpose of reading the past history of the globe is that of the stratified formations.

Studying these stratified formations therefore in greater detail, we find that they fall naturally in their turn into two sets—*viz.*, a mechanical set of pebble beds, sandstones and clays formed of rock fragments washed off the land into the waters, and an organic set of limestones, chalk, &c., formed of the shells and exuviæ of marine organisms.

But when we attempt a further division of these two sets our classification soon begins to lose its definiteness. We infer that some formations, such as the Old Red and the Triassic, were the comparatively rapid deposits of lakes and inland seas; that others, like the Coal Measures, London Clay, &c., were the less rapid deposits of lagoons, river valleys, deltas, and the like; that others, like our finely laminated shales and clays of the Silurian and Jurassic, were the slower deposits of the broader

seas; and finally, that others, like our Chalk and Greensand, were possibly the extremely slow deposits of the more oceanic deeps.

Nevertheless, after looking at the formations collectively, there remains no doubt whatever in the mind of the geologist that their mechanical members are the results of the aqueous degradation of vanished lands, and that their organic members are the accumulated relics of the stony secretions of what once were living beings. Neither is there any possibility of escape from the conclusion that they have all been deposited by water in the superficial hollows of the sea-bottoms and ocean floors of the earth-crust of their time.

In the life of every individual stratified formation of the mechanical type we can always distinguish three stages: first, the stage of erosion and transportation, in which the rock fragments were worn off the rocks of the higher ground and washed down by rain and rivers to the sea; second, a stage of deposition and consolidation below the surface of the quiet waters; and third, a final stage in which the completed rock-formation was bent and upheaved, in part at least, into solid land. In the formations of the organic type three corresponding stages are equally discernible: first, the period of mineral secretion by organized beings; second, the period of deposition and consolidation; and third, the final period of local elevation in mass. But one and all, mechanical and organic alike, they bear in their composition, in their arrangement, and in their fossils, abundant and irresistible evidences that they *were* the products, and that now they *are* the memorials of the physical geography of their time.

Guided by the principles of Hutton and Lyell, geologists have worked out with great care and completeness the effects of those agencies which rule in the first of these three life-stages in the history of a mechanical formation. No present geological processes are better known to the young geologist than those of denudation, erosion, and transportation, so familiar to us in the eloquent works of our President. They form together the subject-matter of that most wonderful fascinating chapter in geology, which from its modest opening among the quiet Norfolk sandhills sweeps upwards and onwards without a break to its magnificent close on the brink of the gorge of the Colorado. But our knowledge of the detailed processes of deposition and consolidation which rule in the second stage is still exceedingly imperfect, although a flood of light has been thrown upon the subject by the brilliant results of the *Challenger* expedition. And we are compelled to admit that our knowledge of the operations of those agencies which rule in the processes of upheaval and depression is as yet almost nil; and what little we have already learnt of the effects of those agencies is the prey of hosts of conflicting theories that merely serve to annoy and bewilder the working student of the science.

But not one of the formative triad of detrition, deposition, and elevation can exist without the others. No detrition is possible without the previous upheaval of the rock-sheet, from which material can be removed; no deposition is possible without the previous depression of the rock-sheet, which forms the basin in which the fragmentary material can be laid down.

Our knowledge, therefore, of the origin and meaning of any geological formation whatever, can at most be only fragmentary until this third chapter in the life-history of the geological formation has been attacked in earnest.

Now all the rich store of knowledge that we possess respecting the first stage in the life of a geological formation has been derived from a comparison of certain phenomena which the stratigraphical geologist finds in the rock formations of the past, with correspondent phenomena which the physical geographer discovers on the surface of the earth of the present. And all that we know of the second stage again has been obtained in precisely the same way. Surely analogy and common sense both teach us that all which is likely to be of permanent value to us as regards the final stage of elevation and depression must first be sought for in the same direction.

Within the last twenty years or so many interesting and vital discoveries have been made in the stratigraphy of the rock formations, which bear largely upon this obscure chapter of elevation and depression. And I propose on this occasion that we try to summarize a few of these new facts, and then, reading them in conjunction with what we actually know of the physical geography of the present day, try to ascertain how such mutual agreement as we can discover may serve to aid the stratigraphical geologist in his interpretation of the true meaning of the geological formations themselves. We may not hope for many years to come to read the whole of this geological chapter, but we may perhaps modestly essay an interpretation of one or two of the opening paragraphs.

In the physical geography of the present day we find the exterior of our terraqueous globe divided between the two elements land and water. We know that the solid geological formations exist everywhere beneath the visible surface of the lands, but of their existence under the present ocean floor we have as yet no absolute certainty. We know both the form of the surface and the composition of the outer layers of the continental parts of the lithosphere; we only know as yet even in outline the form of the surface of its oceanic portions. The surface of each of our great continental masses of land resembles that of a long and broad arch-like form, of which we see the simplest type in the New World. The surface of the North American arch is sagged downwards in the middle into a central depression which lies between two long marginal plateaux, and these plateaux are finally crowned by the wrinkled crests which form its modern mountain systems. The surface of each of our ocean floors exactly resembles that of a continent turned upside down. Taking the Atlantic as our simplest type, we may say that the surface of an ocean basin resembles that of a mighty trough or syncline, buckled up more or less centrally into a medial ridge, which is bounded by two long and deep marginal hollows, in the cores of which still deeper grooves sink to the profoundest depths. This complementary relationship descends even to the minor features of the two. Where the great continental sag sinks below the ocean level, we have our gulfs and our Mediterraneans, seen in our type continent as the Mexican Gulf and Hudson Bay. Where the central oceanic buckle attains the water-line, we have our oceanic islands, seen in our type ocean as St. Helena and the Azores. Although these apparent crust-waves are neither equal in size nor symmetrical in form, this complementary relationship between them is always discernible. The broad Pacific depression seems to answer to the broad elevation of the Old World—the narrow trough of the Atlantic to the narrow continent of America.

Every primary wave of the earth's surface is broken up into minor waves, in each of which the ridge and its complementary trough are always recognizable. The compound ridge of the Alps answers to the compound Mediterranean trough; the continous western mountain chain of the Americas to the continuous hollow of the Eastern Pacific which bounds them; the sweep of the crest of the Himalaya to the curve of the Indo-Gangetic depression. Even where the surface waves of the lithosphere lie more or less buried beneath the waters of the ocean and the seas, the same rule always obtains. The island chains of the Antilles answer to the several Caribbean abysses, those of the Ægean Archipelago answer to the Levantine deeps.

Draw a section of the surface of the lithosphere along a great circle in any direction, the rule remains always the same: crest and trough, height and hollow, succeed each other in endless sequence, of every gradation of size, of every degree of complexity. Sometimes the ridges are continental, like those of the Americas; sometimes orographic, like those of the Himalaya; sometimes they are local, like those of the English Weald. But so long as we do not descend to minor details we find that every line drawn across the earth's surface at the present day rises and falls like the imaginary line drawn across the surface of the waves of the ocean. No rise of that line occurs without its complementary depression; the two always go together, and must of necessity be considered together. Each pair constitutes one of those *geographical units of form* of which every continuous direct line carried over the surface of the lithosphere of our globe is made up. This unit is always made up of an arch-like rise and a trough-like depression, which shade into each other along a middle line of contrary curvature. It resembles the letter S or Hogarth's line of beauty, and is clearly similar in form to the typical wave of the physicist. Here, then, we reach a very simple and natural conclusion, viz. the surface of the earth-crust of the present day resembles that of a series of crust-waves of different lengths and different amplitudes, more or less irregular and complex, it is true, but everywhere alternately rising and falling in symmetrical halves like the waves of the sea.

Now this rolling wave-like earth-surface is formed of the outcropping edges of the rock formations which are the special

objects of study of the stratigraphical geologist. If, therefore, the physiognomy of the face of our globe is any real index of the character of the personality of the earth-crust beneath it, these collective geographical features should be precisely those which answer to the collective structural characters of the geological formations.

In the earlier days of geology one of the first points recognized by our stratigraphists was the fact that the formations were successive lithological sheets, whose truncated outcropping edges formed the present surface of the land, and that these sheets lay inclined at an angle one over the other, or as William Smith quaintly expressed it, like a tilted "pile of slices of bread and butter." But as discovery progressed the explanation of this arrangement soon became evident. The formations revealed themselves as a series of what had originally been deposited as horizontal sheets, lying in regular order one over the other, but which had been subsequently bent up into alternating arches and troughs (*i.e.* the anticlines and synclines of the geologist). Their visible parts, which now constitute the surface of our habitable lands, were simply those parts of the formations which are cut by the irregular plane of the present earth's surface. All those parts of the great arches and troughs formerly occurring above that plane have been removed by denudation; all those parts below that plane lie buried still, out of sight within the solid earth-crust.

Although in every geological section of sufficient extent it was seen that the anticline or arch never occurred without the syncline or trough—in other words, that there was never a rise without a corresponding fall of the stratum, yet it is only of late years that the stratigraphical geologist has come clearly to recognise the fact that the anticline and syncline must be considered together, and must be united as a single crust-wave. For the arch is never present without its complementary trough, and the two together constitute the *tectonic*, *structural*, or *orographic unit*, namely, *The Fold*, the study of which, so brilliantly inaugurated by Heim in his "Mechanismus der Gebirgsbildung," is destined, I believe, in time, to give us the clue to the laws which rule in the local elevation and depression of the earth-crust, and furnish us with the means of discovery of the occult causes which lie at the source of those superficial irregularities which give to the face of our globe its variety, its beauty, and its habitability.

We have said already that this wave or fold of the geologist resembles that of the wave of the physicist. Now we may regard such a wave as formed of two parts, the arch-like part above and the trough-like part below. The length of the wave is naturally the length of the axial line joining the outer extremities of the arch and trough, and passing through the centre, node, or point of origin of the wave itself, which bisects the line of contrary curvature. The amplitude of the wave is the height of the arch added to the depth of the trough. The arch part of such a wave, if perfectly symmetrical, may clearly be regarded as belonging either to a wave travelling to the right, in which case the complementary trough is the one in that direction, or it may be regarded as belonging to a wave travelling to the left, in which case its trough must be the one in that direction. But as in the case of the shore wave, the advancing slope of the wave is always the steeper, and the real centre of the wave must lie half-way down this steeper slope; so there is no difficulty in recognizing the centre of a geological fold and fixing the real direction of movement.

The fold of the geologist differs from the ordinary wave of the physicist, essentially in the fact that even in its most elementary conception, as that of a plate bent by a pressure applied from opposite sides, it necessarily includes the element of thickness. And this being the case, the rock sheet which is being folded and curved has different layers of its thickness affected differently. In the arch of the fold the upper layers of the rock sheet are extended, while its lower layers are compressed. On the contrary in the trough of the fold the upper layers are compressed and the lower layers are extended. But in arch and trough alike there exists a central layer, which, beyond taking up the common wave-like form, remains practically unaffected.

But the geological fold has in addition to length and thickness, the further element of breadth, and this fact greatly complicates the phenomena.

Many of the movements which take place in a rock sheet which is being folded, or in other words those produced by the bending of a compound sheet composed of many leaves, can be fairly well studied in a very simple experiment. Take an ordinary large note-book, say an inch in thickness, with flexible covers. Rule carefully a series of parallel lines across the edges of the leaves at the top of the book, about ¼ of an inch apart, and exactly at right angles to the plane of the cover. Then, holding the front edges loosely, press the book slowly from back and front into an S-like form until it can be pressed no further. As the wave grows, it will be noticed that the cross lines which have been drawn on the upper edge of the book remain fairly parallel throughout the whole of the folding process, except in the central third of the book, where they arrange themselves into a beautiful sheaf-like form, showing how much the leaves of the book have sheared or slidden over each other in this central portion. It will also be seen when the S is complete that the book has been forced into a third of its former breadth. It is clear that the wave which the book now forms must be regarded as made up of three sections: viz. a section forming the outside of the trough on the one side, and a section forming the outside of the arch on the other, and a central or common section, which may be regarded either as uniting or dividing the other two.

As this experiment gives us a fair representation of what takes place in a geological fold, we see at a glance that the geologist is forced to divide his fold into three parts—an arch limb, a trough limb, and a middle limb—which last we may call the *copula* or the *septum*, according as we regard it as connecting or dividing the other two. Our note-book experiment shows us also that in the trough limb and the arch limb the leaves or layers undergo scarcely any change of relative position beyond taking on the growing curvature of the wave. But the layers in the central part, or *septum*, undergo sliding and shearing. It will be found also, by gripping the unbound parts of the book firmly and practising the folding in different ways, that this *septum* is also a region of warping and twisting. This simple experiment should be practised again and again until all these points are apparent, and the various stages of the folding process become clear; the surface of the book being forced first into a gentle arch-like rise with a corresponding trough-like fall; then stage by stage the arch should be pushed over on to the trough until the surfaces of the two are in contact and the book can be folded no further.

In the structure of our modern mountain ranges we discover the most beautiful illustrations of the bending and folding of the rocky formations of the earth-crust. The early results of Rogers among the Alleghanies, of Lory and Favre in the Western Alps, have been greatly extended of late years by the discoveries of Heim and Baltzer in the Central Alps, of Bertrand in Provence, of Margerie in Languedoc, of Dutton and his colleagues in the western ranges of America, and of Peach and Horne and others in the older rocks of Britain. The light these researches throw upon the phenomena of mountain structure will be found admirably summarized and discussed in the works of Leconte, of Dana, of Daubrée, of Reade, of Heim, and finally in the magnificent work of Suess, the "Antlitz der Erde," of which only the first two volumes have yet appeared.

Looking first at the mountain fold in its simplest form as that of a bent rock-plate, composed of many layers which have been forced into two similar arc-like forms, the convexities of which are turned, the one upwards and the other downwards, we find in the present mountain ranges of the globe every kind represented. We commence with one in which the arch is represented merely by a gentle swell of the rock sheet, and the trough by an answering shallow depression, the two shading into each other in an area of contrary flexure. From this type we pass insensibly to others in which we see that the sides of the common limb or septum are practically perpendicular. From these we pass to folds in which the twisted common limb or septum overhangs the vertical, and so on to that final extreme, where the arch limb has been pushed completely over on to the trough limb, and all three members, as in our note-book experiment, are practically welded into one conformable solid mass.

Although the movements of these mountain folds are slow and insensible, and only effected in the course of ages, so that little or no evidence of the actual movement of any single one of them has been detected since they were first studied, yet it is perfectly plain that when we regard them collectively, we have here crust folds in every stage of their existence. Each example in itself represents some one single stage in the lifetime of a single fold. They are simply crust folds of different ages. Some are, as it were, just born; others are in their earliest youth. Some have attained their majority, some are in the prime of life, and some

are in the decrepit stages of old age. Finally, those in which all three members—arch limb, trough limb, and septum—are crushed together into a conformable mass, are dead. Their life of individual movement is over. If the earth pressure increases the material which they have packed together may of course form a passive part of a later fold, but they themselves can move no more.

In many cases, due partly to the action of longitudinal pressures, the septum becomes reduced to a *plane* of contrary motion, namely—the over-fault, or thrust-plane, and the arch limb and the trough limb slide past each other as two solid masses. But here we have no longer a fold, but a fault.

We see that every mountain fold commences first as a gentle alternate elevation and depression of one or more of the component sheets of the geological formations which make up the earth-crust. This movement is due apparently to the tangential thrusts set up by the creeping together, as it were, of those neighbouring and more resistant parts of the earth-crust which lie in front of and behind the moving wave. Yielding slowly to these lateral thrusts the crest of the fold rises higher and higher, the trough sinks lower and lower, the central common limb or septum grows more and more vertical and becomes more and more strained, sheared, and twisted. As this middle limb yields, the rising arch part of the fold is forced gradually over on to the sinking trough, until at last all three members come into conformable contact and further folding as such is impossible. Movement ceases, the fold is dead.

We see also from our note-book experiment that the final result of the completion of the fold is clearly to strengthen up and consolidate that part of the crust plate to the local weakness of which it actually owed its origin and position. The fold has by its life-action theoretically trebled the thickness of that part of the earth-plate in which its dead remains now lie. If the lateral pressure goes on increasing and the layers of the earth-crust again begin to fold in the same region, the inert remains of the first fold can only move as a passive part of a newer fold: either as a part of the new arch-limb, the new trough-limb, or the new septum. As each younger and younger fold formed in this way necessarily includes a more resistant, and therefore a thicker, broader, and deeper sheet of the earth-crust, we have here the phylogenetic evolution of a whole family of crust folds, each successive member of which is of a higher grade than its immediate predecessor.

But it very rarely happens that the continuous plate in which any fold is imbedded is able to resist the crust creep until the death of the first fold. Usually, long before the first simple fold is completed, a new and a parallel one rises in front of it on the side of the trough limb, and the two grow, as it were, henceforward side by side. But the younger fold, being due to a greater pressure than the older, must of necessity be of a higher specific grade, and the two together form a generic fold in common.

Our present mountain systems are all constituted of several families of folds, all formed in this way, of different gradations of size, of different dates of origin, and of different stages of life evolution; and in each family group the members are related to each other by this natural genetic affinity.

Sometimes the new folds are formed in successive order on one side of the first fold, and then we have our unilateral (or so-called unsymmetrical) mountain groups, like those of the Jura and the Bavarian Alps. Sometimes they are formed on both sides of the original fold, and then we have our bilateral (or so-called symmetrical) ranges, like the Central Alps. In both cases the septa of the aged or dead folds are of necessary all directed inwards towards the primary fold. If, therefore, they originate only on one side of the fold, our mountain group looks unsymmetrical, with a very steep side opposed to a gently sloping side. If they grow on both sides of the original fold, we have the well-known "fan structure" of mountain ranges. In this case the whole complex range is seen at a glance to be a vast compound arch of the upper layers of the earth-crust, keyed up by the material of the dead or dying folds, which by the necessities of the case constitute mighty wedges whose apices are directed inwards towards the centre of the system. But a complete arch of this kind is in reality not a single fold, but a double one, with a septum on both sides of it; and it requires two troughs, one on each side of it, as its natural complement. The so-called unsymmetrical ranges, therefore, which are constituted merely of arch limb, trough limb, and septum, are locally the more natural and the more common.

It is clear that in the lifetime of any single fold its period of greatest energy and most rapid movement must be that of middle life. In early youth the lateral pressure is applied at a very small angle, and the tangential forces act therefore under the most disadvantageous circumstances. In the middle life of the fold the arch limb and the trough limb stand at right angles to the septum, and the work of deformation is then accomplished under the most favourable mechanical conditions and with the greatest rapidity. That is to say, the activity of the fold and the rate of movement of the septum, like the speed of the storm wind, varies directly as the gradient.

In our note-book experiment we observed that little or no change took place in the arch limb and trough limb, while the septum became remarkably sheared and twisted. The same is the case in nature, but here we have to recollect that these moving mountain folds are of enormous size, indeed actual mountains in themselves. These great arches, scores of miles in length, thousands of feet in height and thickness, must of necessity be of enormous weight, capable of crushing to powder the hardest rocks over which they move, while the thrust which drives them forward is practically irresistible. It is plain, therefore, that while the great arch limb and the trough limb of one of these mighty folds move over and under each other from opposite directions, they form together an enormous machine, composed of two mighty rollers or millstones, which mangle, roll, tear, squeeze, and twist the rocky material of the middle limb or septum, which lies jammed in between them, into a laminated mass. This deformed material, which is the characteristic product of the mountain-making forces, is, of course, made up of the stuff or the original middle limb of the fold; and whether we call it breccia, mylonite, phyllite, or schist, although it may be composed of sedimentary stuff, it is certainly no longer a *stratified* rock; and though it may have been originally purely igneous material, it is certainly no longer *volcanic*. It is now a manufactured article made in the great earth mill.

These mountain folds, however, are merely the types of folds and wrinkles of all dimensions which affect the rock formations of the earth-crust. Within the mountain chains themselves we can follow them fold within fold, first down to formations, then to strata, then to laminæ, till they disappear at last in microscopic minuteness beyond the limits of ordinary vision. Leaving these, however, for the moment, let us travel rather in the opposite direction, for these mountain folds are by no means the largest known to the stratigraphical geologist. Look at any geological section crossing the continent of North America, and it will be found that the whole of the Rocky Mountain range on its western side and the Alleghany range on the east are really two mighty compound geological anticlines, while the broad sag of the Mississippi Basin is actually a compound geological syncline made up of the whole pile of the geological formations. That is to say, the continent of North America is composed of a pair of geological folds, the two arches of which are represented by the Rockies on the one side and the Alleghanies on the other, while the intermediate Mississippi syncline is the common property of both. Here, then, we reach a much higher grade of fold than the orographic or mountain-making fold, viz. the plateau-making fold or the semi-continental fold, which, because of its enormous breadth, must include a very much thicker portion of the earth-crust than the ordinary orographic fold itself.

But which must be the real middle limbs of these two American folds, those septal areas where most work is being done and the motion is greatest?

Taught by what we have already learned of the mountain wave the answer is immediate and certain. They must be on the steeper sides of each of the two folds, namely, those which face the ocean. How perfectly this agrees with the geological facts goes without saying. It is on the steep Pacific side of the western fold that the crushing and crumpling of its rocks is the greatest. It is on the Atlantic side of the eastern fold that the contortion and the metamorphism of its rocks are at their maximum, while in the common and gently sloping trough of both folds, namely, the intermediate Mississippi Valley, the entire geological sequence remains practically unmodified throughout.

Again, which of these two American folds should be the more active at the present day? Taught by our study of the mountain wave the answer again is immediate and conclusive. It must be that fold whose septum has the steeper gradient. Geology and geography flash at once into combination. The steeper Pacific septum of the western fold from Cape Horn almost to

Alaska is ablaze with volcanoes, while the gently inclined Atlantic septum of the eastern fold from Greenland to Magellan Straits shows none, except on the outer edge of the Antilles, in the very region where the slope of the surface is the steepest. We see at a glance that the vigour of these two great continental folds, like those of our mountain waves, varies directly as the surface gradient of the septum.

But the geographical surface of North America, considered as a whole, is in reality that of a double arch, with a sag or common trough in the middle. We have seen already that this double arch must be regarded as the natural complement of the equally double Atlantic trough. Here, then, if the path of analogy we have hitherto so triumphantly followed up to this point is still to guide us, the basin of the Atlantic must be, not only in appearance, but in actuality, formed of two long minor folds of the same grade as the two that form the framework of America, but with their members arranged in reverse order. If so, their submarine septa ought also to be lines of movement and of volcanic action. And this is again the case. The volcanic islands of the Azores and St. Helena lie not exactly on the longitudinal crests of the mid-oceanic *Challenger* ridge, but upon its bounding flanks.

But we have not yet, however, finished with our simple fold. If we draw a line completely round the globe, crossing the Atlantic basin at its shallowest, between Cape Verde to Cape St. Roque, and continued in the direction of Japan, where the Pacific is at its deepest, as the trace of a great circle we find that we have before us a crust fold of the very grandest order. We have one mighty continental arch stretching from Japan to Chili, broken medially by the sag of the Atlantic trough, and this great terrestrial arch stands directly opposed to its natural complement, the great trough of the Pacific, which is bent up in the middle by the mightiest of all the submarine buckles of the earth-crust, on which stand the oceanic islands of the central Pacific.

But if this be true, then the septum of all septa on our present earth-crust must cross our grandest earth fold where the very steepest gradient occurs along this line, and it must constitute the centre-point of the moving earth fold, and of greatest present volcanic activity. And where is this most sudden of all depressions? Taught once more by our geological fold, the answer is instantaneous and incontrovertible. It is on the shores of Japan, the region of the mightiest and most active of all the living and moving volcanic localities on the face of our globe.

But the course of the line which we indicated as forming our grandest terrestrial fold returns upon itself. It is an endless fold, an endless band, the common possession of two sciences. It is geological in origin, geographical in effect. It is the *wedding-ring* of geology and geography, uniting them at once and for ever in indissoluble union.

Such an endless fold, again, must have an endless septum, which, in the nature of things, must cross it twice. Need I point out to the merest tyro in these wedded sciences that if we unite the Old and New Worlds and Australia, with their intermediate sags of the Antarctic and Indian Oceans, as one imperial earth arch, and regard the unbroken watery expanse of the Pacific as its complementary depression, then the circular coastal band of contrary surface flexure between them should constitute the moving master septum of the earth crust. This is the "Volcanic girdle of the Pacific," our "Terrestrial Ring of Fire."

Or, finally, if we rather regard the compact arch of the Old World itself as the natural complement of the broken Indo-Pacific depression, then the most active and continuous septal band of the present day should divide them. Again our law asserts itself triumphantly. It is the great volcanic and earthquake band on which are strung the Festoon Islands of Western Asia, the band of Mount St. Elias, the Aleutians, Kamtchatka, the Kuriles, the band of Fusijama, Krakatoa, and Sangir. The rate of movement of the earth's surface doubtless everywhere varies directly as the gradient.

We find, therefore, that even if we restrict our observations to the most simple and elementary conception of the rock fold as being made up of arch-limb, trough-limb, and twisting but still continuous septum, we are able to connect, in one unbroken chain, the minutest wrinkle of the finest lamina of a geological formation with the grandest geographical phenomena on the face of our globe.

We find, precisely as we anticipated, that the wave-like surface of the earth of the present day reflects in its entirety the wave-like arrangement of the geological formations below. On the land we find that the surface arches and troughs answer precisely to the grander regional anticlines and synclines of the subterranean sedimentary sequence; and it may, I believe, be regarded as certain that the submarine undulations have a similar or complementary relationship. We find in the new geology, as Hutton found in the old, that geography and geology are one. We find, as we suspected, that the physiognomy of the face of our globe is an unerring index of the solid personality beneath. It bears in its lineaments the characteristic family features and the common traits of its long line of geological ancestors.

Such, it seems to me, is an imperfect account of the introductory paragraphs of that great chapter in the New Geology now in course of interpretation by geologists of the present day; and we have translated them exactly in the old way by the aid of the only living geological language, the language of present natural phenomena, and I doubt not that sooner or later the rest of this great chapter will be read by the same simple means.

I have confined myself to-day to the discussion of the characteristics of the simple geological fold as reduced to its most elementary terms of arch, trough, and unbroken septum; for this being clearly understood, the rest naturally follows. But this twisted plate is really the key which opens the entire treasure-house of the New Geology in which lie spread around in bewildering confusion facts, problems, and conclusions enough to keep the young geologist and other scientific men busily at work for many a long year to come.

Into this treasure-house I often wander myself, in the few leisure hours that I can steal from a very busy professional life; and out of it I bring now and again heresies that sometimes amuse and sometimes horrify my geological friends. As you have so patiently listened to what I have already said, perhaps you will permit me in a few final sentences to indicate in brief some of those novelties which I see already more or less clearly, and a few of those less novel points on which it appears to me that more light is wanted. My excuse is twofold—first, to furnish material for work and controversy to the young geologists; and second, to obtain aid for myself from workers in other walks of science.

The account of the simple rock-fold I have already given you is of the most elementary kind. It presupposes merely the yielding to tangential pressure from front and back, combined with effectual resistance to sliding. But in the layers of the earth-crust there is always, in addition, a set of tangential pressures theoretically at right angles to this. The simple fold becomes a *folded fold*, and the compound septum twists not only vertically but laterally. On the surface of the globe this double set of longitudinal and transverse waves is everywhere apparent. They account for the detailed disposition of our lands and our waters, for our present coastal forms, for the direction, length, and disposition of our mountain-ranges, our seas, our plains, and lakes. The compound arch becomes a dome, its complementary trough becomes a basin. The elevations and depressions, major and minor, are usually twinned, like the twins of the mineralogist, the complementary parts being often inverted, and turned through 180° (compare Italy with the Po-Adriatic depression). Every upward swirl and eddy has its answering downward swirl. The whole surface of our globe is thus broken up into fairly continuous and paired masses, divided from each other by moving areas and lines of mountain making and crust movement, so that the surface of the earth of the present day seems to stand midway in its structure and appearance between those of the sun and the moon, its eddies wanting the mobility of those of the one and the symmetry of those of the other. In the geology of the earth-crust, also, the inter-crossing of the two sets of folds, theoretically at right angles to each other, gives rise to effects equally startling. It lies at the origin of the thrust-plane or overfault, where the septal region of contrary motion in the fold becomes reduced to, or is represented by, a *plane* of contrary motion. It allows us to connect together under one set of homologies folds and faults. The downthrow side of the fault answers to the trough, the upthrow side to the arch, of our longitudinal fold; while the fault-plane itself represents the septal area reduced to zero. The node of the fault, and the alternation and alteration of throw, are due to the effects of the transverse folding.

These transverse folds of different grades, which affect different layers of the earth-crust differentially, account also for the formation of laccolites, of granitic cores, and of petrological provinces; and they enable us also to understand many of the phenomena of metamorphism.

Of the folds of the third order I shall here say nothing; but I must frankly admit that the primal cause of all this tangential movement and folding stress is still as mysterious to me as ever. I incline to think that it is due to many causes—tidal action, sedimentation, and many others. I cannot deny, however, that it may be *mainly* the result of the contraction in diameter of our earth, due to the loss of its original heat into outer space? For everywhere we find evidences of symmetrical crushing of the earth-crust by tangential stresses. Everywhere we find proofs that different layers of that crust have been affected differentially, and the outer layers have been folded the most. We seem to be dealing not so much with a solid globe as with a globular shell composed of many layers.

Is it not just possible after all that, as others have suggested, our earth is such a hollow shell, or series of concentric shells, on the surface of which gravity is at a maximum, and in whose deepest interior it is non-existent? May this not be so also in the case of the sun, through whose spot-eddies we possibly look into a hollow interior? If so, perhaps our present nebulæ may also be hollow shells formed of meteorites; on the surfaces of these shells the fiery spirals we see would be the swirls which answer to the many twisting crustal septa of the earth. Our comets, too, in this case might be elongated ellipsoids, whose visible parts would be merely interference phenomena or sheets of differential movement.

In this case we have represented before us to-day all the past of our earth as well as its present. Uniformity and evolution are one.

Thus from the microscopic septa of the laminæ of the geological formations we pass outwards *in fact* to these moving septa of our globe, marked on land by our new mountain-chains, and on our shores by our active volcanoes. Thence we sweep, *in imagination*, to the fiery eddies of the sun, and thence to the glowing swirls of the nebulæ; and so outwards and upwards to that most glorious septum of all the visible creation, the radiant ring of the Milky Way.

Prof. George Darwin, in his address to the section of mathematical and physical science at the meeting of the British Association at Birmingham in 1886, with all the courage of genius, and the authority of one of the sons of the prophets, acknowledged that it seems as likely that "meteorology and geology will pass the word of command to cosmical physics as the converse." Behind this generous admission I shelter myself. But I feel absolutely confident that long after the physicists may have swept away these provisional astronomical suggestions as "the baseless fabric of a vision," there will still remain in the treasure-house of the geological fold a wealth of abundant material for the use of the mathematician, the physicist, the chemist, the mineralogist, and the astronomer, of the deepest interest and of the highest value.

SECTION H.

ANTHROPOLOGY.

OPENING ADDRESS BY ALEXANDER MACALISTER, M.D., F.R.S., PROFESSOR OF ANATOMY IN THE UNIVERSITY OF CAMBRIDGE, PRESIDENT OF THE SECTION.

ON an irregular and unfenced patch of waste land, situated on the outskirts of a small town in which I spent part of my boyhood, there stood a notice board bearing the inscription, "A Free Coup," which, when translated into the language of the Southron, conveyed the intimation, "Rubbish may be shot here." This place, with its ragged mounds of unconsidered trifles, the refuse of the surrounding households, was the favourite playground of the children of the neighbourhood, who found a treasury of toys in the broken tiles and oyster-shells, the crockery and cabbage-stalks, which were liberally scattered around. Many a make-believe house and road, and even village, was constructed by these mimic builders out of this varied material, which their busy little feet had trodden down until its undulated surface assumed a fairly coherent consistence.

Passing by this place ten years later I found that its aspect had changed; terraces of small houses had sprung up, mushroom-like, on the unsavoury foundation of heterogeneous refuse. Still more recently I notice that these in their turn have been swept away, and now a large factory, wherein some of the most ingenious productions of human skill are constructed, occupies the site of the original waste.

This commonplace history is, in a sense, a parable in which is set forth the past, present, and possible future of that accumulation of lore in reference to humanity to which is given the name Anthropology, and for the study of which this Section of our Association is set apart. At first nothing better than a heap of heterogeneous facts and fancies, the leavings of the historian, of the adventurer, of the missionary, it has been for long, and alas is still, the favourite playground of *dilettanti* of various degrees of seriousness. But upon this foundation there is rapidly rising a more comely superstructure, fairer to see than the original chaos, but still bearing marks of transitoriness and imperfection, and I dare hazard the prediction that this is destined in the course of time to give place to the more solid fabric of a real Science of Anthropology.

We cannot yet claim that our subject is a real science in the sense in which that name is applied to those branches of knowledge, founded upon ascertained laws, which form the subjects of most of our sister Sections; but we can justify our separate existence, in that we are honestly endeavouring to lay a definite and stable foundation, upon which in time to come a scientific Anthropology may be based.

The materials with which we have to do are fully as varied as were those in my illustration, for we as anthropologists take for our motto the sentiment of Chremes, so often quoted in this Section, *humani nihil a nobis alienum putamus*, and they are too often fully as fragmentary. The bones, weapons, and pottery which form our only sources of knowledge concerning prehistoric races of men, generally come to us as much altered from their original forms as are the rusty polyhedra which once were the receptacles for biscuits or sardines. The traditions, customs, and scraps of folk-lore which are treasures to the constructive anthropologist, are usually discovered as empty shells, in form as much altered from their original conditions as are those smooth fragments of hollow white cylinders which once held the delicate products of the factory of Keiller or Cairns.

I have said that Anthropology has not yet made good its title to be ranked as an independent science. This is indicated by the difficulty of framing a definition at the same time comprehensive and distinctive. Mr. Galton characterizes it as the study of what men are in body and mind, how they came to be what they are, and whither the race is tending; General Pitt-Rivers, as the science which ascertains the true causes for all the phenomena of human life. I shall not try to improve upon these definitions, although they both are manifestly defective. On the one side our subject is a branch of biology, but we are more than biologists compiling a monograph on the natural history of our species, as M. de Quatrefages would have it. Many of the problems with which we deal are common to us and to psychologists; others are common to us and to students of history, of sociology, of philology, and of religion; and, in addition, we have to treat of a large number of other matters æsthetic, artistic, and technical, which it is difficult to range under any subordinate category.

In view of the encyclopædic range of knowledge necessary for the equipment of an accomplished anthropologist, it is little wonder that we should be, as we indeed are, little better than smatterers. Its many-sided affinities, its want of definite limitation, and the recent date of its admission to the position of an independent branch of knowledge, have hitherto caused Anthropology to fare badly in our Universities. In this respect, however, we are improving, and now in the two great English Universities there are departments for the study of the natural history of man and of his works.

Out of the great assemblage of topics which come within our sphere, I can only select a few which seem at present to demand special consideration. The annual growth of our knowledge is chiefly in matters of detail which are dull to chronicle, and the past year has not been fertile in discoveries bearing on those great questions which are of popular interest.

On the subject of the antiquity of man there are no fresh discoveries of serious importance to record. My esteemed predecessor at the Leeds meeting two years ago, after reviewing

the evidence as to the earliest traces of humanity, concluded his survey with the judgment, "On the whole, therefore, it appears to me that the present verdict as to tertiary man must be in the form of 'Not Proven.'" Subsequent research has not contributed any new facts which lead us to modify that finding. The most remarkable of the recent discoveries under this head is that of the rude implements of the Kentish chalk-plateau described by Professor Prestwich; but while these are evidently of archaic types, it must be admitted that there is even yet room for difference of opinion as to their exact geological age.

Neither has the past year's record shed new light on the darkness which enshrouds the origin of man. What the future may have in store for us in the way of discovery we cannot forecast; at present we have nothing but hypothesis, and we must still wait for further knowledge with the calmness of philosophic expectancy.

I may, however, in this connection refer to the singularly interesting observations of Dr. Louis Robinson on the prehensile power of the hands of children at birth, and to the graphic pictures with which he has illustrated his paper. Dr. Robinson has drawn, from the study of the one end of life, the same conclusion which Mr. Robert Louis Stevenson deduced from the study of his grandfather, that there still survive in the human structure and habit traces of our probably arboreal ancestry.

Turning from these unsolved riddles of the past to the survey of mankind as it appears to us in the present, we are confronted in that wide range of outlook with many problems well nigh as difficult and obscure.

Mankind, whenever and however it may have originated, appears to us at present as an assemblage of tribes, each not necessarily homogeneous, as their component elements may be derived from diverse genealogical lines of descent. It is much to be regretted that there is not in our literature a more definite nomenclature for these divisions of mankind, and that such words as *race*, *people*, *nationality*, *tribe*, and *type* are often used indiscriminately as though they were synonyms.

In the great mass of knowledge with which we deal there are several collateral series of facts, the terminologies of which should be discriminated. In the first place there are those ethnic conditions existing now, or at any other point in time, whereby the individuals of mankind are grouped into categories of different comprehension, as *clans* or families, as *tribes* or groups of allied clans, and as *nations*, the inhabitants of restricted areas under one political organization. This side of our subject constitutes Ethnology.

In the second place, the individuals of mankind may be regarded as the descendants of a limited number of original parents, and consequently each person has his place on the genealogical tree of humanity. As the successive branches became in their dispersion subjected to the influences of diverse environments, they have eventually differentiated in characteristics. To each of these subdivisions of the phylum thus differentiated the name *race* may appropriately be restricted, and the sum of the peculiarities of each race may be termed *race-characters*. This is the phylogenetic side of Anthropology, and its nomenclature should be kept clearly separate from that of the ethnological side. The great and growing literature of Anthropology consists largely of the records of attempts to discover and formulate these distinctive race-characters. *Race* and *tribe* may be terms of equal extension, but the standpoint from which these categories are viewed is essentially different in the two cases.

There is yet a third series of names in common use in Descriptive Anthropology. The languages in use among men are unfortunately numerous, and as the component individuals in each community usually speak a common language, the mistake is often made of confounding the tribal name with that of the tribal language. Sometimes these categories are co-extensive; but it is not always so, for it is a matter of history that communities have been led to adopt new languages from considerations quite independent of phylogenetic or ethnic conditions. These linguistic terms should not be confounded with the names in either of the other series, for, as my learned predecessor once said in a presidential address, it is as absurd to speak of an Aryan skull as it would be to say that a family spoke a brachycephalic language.

In the one clan there may be, by intermarriage, the representatives of different races; in the one nation there may be dissimilar tribes, each derived by composite lines of ancestry from divergent phyla, yet all speaking the same language.

We have an excellent illustration of the confusion resulting from this disregard of precision in the case of the word *Celtic*, a term which has sometimes been employed as an ethnic, sometimes as a phylogenetic, and sometimes as a linguistic species. In the last-named sense, that to which I believe the use of the name should be restricted, it is the appropriate designation of a group of cognate languages spoken by peoples whose physical characters show that they are not the descendants of one common phylum in the near past. There are fair-haired, long-headed families in Scotland and Ireland; fair, broad-headed Bretons; dark-haired, round-headed Welshmen; and dark-haired, long-headed people in the outer Hebrides, McLeans, "Sancho Panza type"—men obviously of different races, who differ not only in colour, stature, and skull-form, but whose traditions also point to a composite descent, and yet all originally speaking a Celtic tongue. The use of the word *Celtic* as if it were the name of a phylogenetic species has naturally led to hopeless confusion in the attempts to formulate race-characters for the Celtic skull—confusions of a kind which tend to bring physical anthropology into discredit. Thus Retzius characterizes the Celtic crania as being dolichocephalic, and compares them with those of the modern Scandinavians. Sir Daniel Wilson considers the true Celtic type of skull as intermediate between the dolichocephalic and the brachycephali; and Topinard figures as the typical Celtic skull that of an Auvergnat, extremely brachycephalic, with an index of 85!

Our traditional history tells that we, the Celtic-speaking races of Britain, are not of one common ancestry, but are the descendants of two distinct series of immigrants, a British and a Gaelic. Whatever may have been the origin of the former, we know that the latter are not homogeneous, but are the mixed descendants of the several Fomorian, Nemedian, Firbolg, Tuatha de Danaan, and Milesian immigrations, with which has been combined in later times a strong admixture of Scandinavian blood. It is now scarcely possible to ascertain to which of these component strains in our ancestry we owe the Celtic tongue which overmastered and supplanted the languages of the other tribes, but it is strictly in accordance with what we know of the history of mankind that this change should have taken place. We have instances in modern times of the adoption by conquered tribes of the language of a dominant invading people. For example, Mr. Hale has lately told us that the speech of the Hupas has superseded the languages of those Californian Indians whom they have subdued. In like manner, nearer home, the English language is slowly but surely supplanting the Celtic tongues themselves.

We may here parenthetically note that what has been observed in the case of language has also taken place in reference to ritual and custom. Observances which have a history and a meaning for one race have, in not a few instances, been adopted by or imposed upon other races to whom they have no such significance, and who in incorporating them give to them a new local colour. These pseudomorphs of the earlier cultures are among the most perplexing of the problems which the student of comparative religion or folk-lore has to resolve.

But we want more than a perfect nomenclature to bring Anthropology into range with the true sciences. We need a broader basis of ascertained fact for inductive reasoning in almost all parts of our subject; we want men trained in exact method who will work patiently at the accumulation, verification, and sorting of facts, and who will not prematurely rush into theory. We have had enough of the untrained writer of papers, the jerry-builder of unfounded hypotheses whose ruins cumber our field of work.

The present position of our subject is critical and peculiar; while on the one hand the facilities for anthropological research are daily growing greater, yet in some directions the material is diminishing in quantity and accessibility. We are accumulating in our museums treasures both of the structure and the works of man, classified according to his distribution in time and space; but at the same time some of the most interesting tribes have vanished, and others are rapidly disappearing or becoming fused with their neighbours. As these pass out of existence we, with them, have lost their thoughts, their tongues, and their traditions; for even when they survive, blended with other races, that which was a religion has become a fragmentary superstition, then a nursery tale or a child's game, and is destined finally to be buried in oblivion. The unifying influences of commerce, aided by steam and electricity, are effectually effacing the landmarks

between people and people, so that if we are to preserve in a form fit for future use the shreds which remain of the myths, folk-lore, and linguistic usages of many of the tribes of humanity, we must be up and doing without delay. It is on this account that systematic research such as that which Mr. Risley has advocated with regard to the different races of India is of such pressing and urgent importance. It is for this reason likewise that we hail with pleasure the gathering of folk-lore while yet it survives, and welcome such societies for the purpose as the Folk-lore Congress recently inaugurated.

I have said that in the department of Physical Anthropology our facilities for research are increasing. The newly-founded anthropometric laboratories are beginning to bring forth results in the form of carefully compiled statistical tables, embodying the fruits of accurate observations, which are useful as far as they go. Were these extended in their scope the same machinery might easily gather particulars as to the physical characters of the inhabitants of different districts, which would enable the anthropologist to complete in a systematic manner the work which Dr. Beddoe had so well begun. I would commend this work to the consideration of the provincial university colleges, especially those in outlying districts.

Of all the parts of the human frame, the skull is that upon which anthropologists have in the past expended the most of their time and thought. We have now, in Great Britain alone, at least four collections of skulls, each of which includes more than a thousand specimens, and in the other great national and university museums of Europe there are large collections available for study and comparison.

Despite all the labour that has been bestowed on the subject, craniometric literature is at present as unsatisfactory as it is dull. Hitherto observations have been concentrated on cranial measurements as methods for the discrimination of the skulls of different races. Scores of lines, arcs, chords, and indexes have been devised for this purpose, and the diagnosis of skulls has been attempted by a process as mechanical as that whereby we identify certain issues of postage-stamps by counting the nicks in the margin. But there is underlying all these no unifying hypothesis, so that when we, in our sesquipedalian jargon describe an Australian skull as microcephalic, phænozygous, tapeino-dolichocephalic, prognathic, platyrhine, hypselopalatine, leptostaphyline, dolichuranic, chamæprosopic, and microseme, we are no nearer to the formulation of any philosophic concept of the general principles which have led to the assumption of these characters by the cranium in question, and we are forced to echo the apostrophe of Von Török, "Vanity, thy name is Craniology."

It was perhaps needful in the early days of the subject that it should pass through the merely descriptive stage; but the time has come when we should seek for something better, when we should regard the skull not as a whole complete in itself, nor as a crystalline geometrical solid, nor as an invariable structure, but as a marvellously plastic part of the human frame, whose form depends on the co-operation of influences, the respective shares of which in moulding the head are capable of qualitative if not of quanitative analysis. Could measurements be devised which would indicate the nature and amounts of these several influences, then, indeed, would craniometry pass from its present empirical condition, and become a genuine scientific method. We are yet far from the prospect of such an ideal system, and all practical men will realize the immense, but not insuperable, difficulties in the way of its formulation.

In illustration of the profound complexity of the problem which the craniologist has to face, I would ask your indulgence while I set out a few details to show the several factors whose influence should be numerically indicated by such a mode of measurement.

The parts composing the skull may be resolved into four sets: there is, first, the brain-case; secondly, the parts which subserve mastication and the preparation of the food for digestion; thirdly, the cavities containing the organs of the senses of hearing, sight, and smell; and fourthly, those connected with the production of articulate speech. If our measurements are to mean anything, they should give us a series of definite numbers indicating the forms, modifications, and relative size of these parts, and their settings with regard to each other and to the rest of the body.

To take the last point first, it needs but a small consideration to show that the parts of the skull are arranged above and below a certain horizontal plane, which is definite (although not easily ascertained) in every skull, human or animal. This is the plane of vision. The familiar lines of Ovid—

> Pronaque cum spectent animalia cetera terram,
> Os homini sublime dedit; cœlumque tueri
> Jussit, et erectos ad sidera tollere vultus—

are anatomically untrue, for the normal quadruped and man alike, in their most natural position, have their axis of vision directed to the horizon. Systems of measurement based upon any plane other than this are essentially artificial. There are at the outset difficulties in marking the plane accurately on the skull, and it is to be deplored that the anthropologists of different nations should have allowed themselves to be affected by extraneous influences, which have hindered their unanimous agreement upon some one definite horizontal plane in craniometry.

The Frankfort plane drawn through the upper margins of the auditory foramina and the lowest points of the orbital borders has the advantage of being easily traced and differs so little from the plane of vision that we may without substantial error adopt it.

The largest part of the skull is that which is at once the receptacle and the protector of the brain, a part which, when unmodified by external pressure, premature synostosis, or other adventitious conditions, owes its form to that of the cerebral hemispheres which it contains. Speaking in this city of George and Andrew Combe, I need not do more than indicate in this matter that observation and experiment have established on a firm basis certain fundamental points regarding the growth of the brain. The study of its development shows that the convolutioning of the cerebral hemisphere is primarily due to the connection, and different rate of growth, of the superficial layer of cells with the underlying layers of white nerve fibres; and that so far from the shape being seriously modified by the constraining influence of the surrounding embryonic skull, the form of the soft membranous brain-case is primarily moulded upon the brain within it, whose shape it may however be, to some extent, a secondary agent in modifying in later growth. We have also learned that, although in another sense from that of the crude phrenology of Aristotle, Porta, or Gall, the cerebrum is not a single organ acting as a functional unit, but consists of parts, each of which has its specific province; that the increase in the number of cells in any area is correlated with an increase in the size and complexity of pattern of the convolutions of that area; and that this in turn influences the shape of the enclosing shell of membrane and subsequently of bone.

The anatomist and the physiologist have worked hand in hand in the delimitation of these several functional areas, and pathology and surgery have confirmed what experimental physiology has taught. The topography of each part of the cerebrum, so important to the operating surgeon, should be pressed into the service of the anthropologist, whose measurements of the brain-case should have definite relation to these several areas. In the discussion which is to take place on this subject, I hope that some such relationships will be taken account of. This is not the place to work out in detail how this may be done; I only desire to emphasize the fundamental principle of the method.

The second factor which determines the shape of the individual skull is the size of the teeth. That these differ among different races is a matter of common observation; thus the average area of the crowns of the upper-jaw teeth in the male Australian is 1,536 sq. mm., while in the average Englishman it is only 1,286 sq. mm., less than 84 per cent. of that size.[1]

It is easy to understand how natural selection will tend to increase the size of the teeth among those races whose modes of feeding are not aided by the cook or the cutler; and how, on the other hand, the progress of civilised habits, assisted by the craft of the dentist, interferes with the action of selection in this matter among the more cultured races.

For larger teeth a more extensive alveolar arch of implantation is necessary; and as the two jaws are commensurately developed, the lower jaw of the macrodontal races exceeds that of the meso- or microdontal races in weight. Thus that of a male Australian exceeds that of the average Englishman in the proportion of 100 : 91.

To work this heavier jaw more powerful muscles are needed.

[1] These and the succeeding averages are from my own measurements, taken from never less than ten individual cases.

In the average well-developed Englishman with perfect teeth the weight of the fleshy portion of the great jaw-muscles, masseters and temporals, is 60 grammes, while the weight of those as ascertained in two Australians was 74 grammes.

Correlated with this greater musculature a sharper definition of the areas for the attachments of the jaw-muscles is required. The muscular fascicles are approximately of uniform size in both microdonts and macrodonts, as the range of motion of the jaw differs little in different races; but when the skull is smaller on account of the smaller size of the brain which it contains, the temporal crest ascends higher on the side-wall. In the average Englishman the temporal crests at their points of greatest approximation anteriorly across the brows are 112 mm. apart, but in the Australian they are only separated by 103 mm.: the interstephanic distances in these two are respectively 132 and 114 mm.

The more powerful stroke of the mandibular teeth upon the anvil of the upper-jaw teeth in macrodonts renders necessarily a proportionally stronger construction of the bases of support for the upper alveolar arch. In any skull this arch requires to be solidly connected to the wall of the brain-case to which the shock of the impact is ultimately transmitted, and in order to protect from pressure the delicate intervening organs of sight and smell, the connection is accomplished by the reversed arches of the infraorbital margins with their piers, malar and maxillary, founded on the frontal angular processes. These foundations are tied together by the strong supraorbital ridge, so that the whole orbital edge is a ring, made up of the hardest and toughest bone in the skeleton.

A twofold modification of this arrangement is required in the macrodont skull. The bony circum-orbital ring becomes stronger, especially along its lateral piers; and also as the alveolar arch is longer, and consequently projects farther forward, its basis of support must be extended to meet and bear the malar and maxillary piers. But macrodonts are often microcephalic, and therefore the frontal region of the skull must be adjusted to form a foundation for this arch. In the average English male skull, held with its visual axes horizontal, a perpendicular dropped from the anterior-surface of the fronto-nasal suture will cut the plane of the alveolar arch between the premolar teeth or through the first premolar. In an Australian skull the perpendicular cuts the horizontal plane at the anterior border of the first molar teeth.

It is obvious, therefore, that to ensure firmness, the piers of the arches must be obliquely set; hence the jaw is prognathous, but it is also needful that the supra-orbital arcade should be advanced to meet and bear these piers, as the mandibular stroke is always vertical.

But the inner layer of the skull is moulded on the small frontal lobes of the brain, so this forward extension must affect only the much thicker and tougher outer table of the skull, which, at the period of the second dentition, here separates from the inner table, the interval becoming lined by an extension of the mucosa of the anterior ethmoidal cell. In this way an air space, the frontal sinus, is formed, whose development is thus directly correlated to the two factors of brain development and size of the teeth. If the frontal lobes are narrow in a macrodont skull, then the foundations of the outer or malar piers of the orbital arch must be extended outwards as well as forwards, the external angular process becoming a prominent abutment at the end of a strong low-browed supraorbital arch, whose overhanging edge gives to the orbital aperture a diminished vertical height.

The crania of the two most macrodont races of mankind, Australian and African, differ in the relation of the jaw to the frontal bone. In the microcephalic Australian, the maxillæ are founded upon the under side of the shelf-like projection of the outer table of the frontal, which juts out as a buttress to bear it. On the other hand the nasal processes of the mesocephalic negro ascend with greater obliquity to abut on the frontal, and have, by their convergence, crushed the nasal bones together, and caused their coalescence and diminution.

The crania of the two most microcephalic races present distinctive features of contrast along the same lines. The Bushman's skull is usually orthognathous, with a straight forehead and a shallow frontonasal recess, while the Australian skull is prognathous with heavy overhanging brows. These conditions are correlated to the mesodontism of the Bushman and the macrodontism of the Australian respectively.

In the course of the examination of the relations of brain development to skull growth, some interesting collateral points are elicited. The frontal bone grows from lateral symmetrical centres, which medially coalesce, union taking place usually between the second and sixth years of age. It has been noticed by anthropologists that metopism, as the anomalous non-union of the halves of this bone has been termed, is rare among microcephalic races, occurring only in about 1 per cent. among Australian skulls. Increased growth of the frontal lobes as the physical accompaniment of increased intellectual activity interposes an obstacle to the easy closure of this median suture, and so in such races as the ancient Egyptian, with a broader forehead, metopism becomes commoner, rising to 7 per cent. In modern civilised races the percentage ranges from 5 to 10. In following out the details of this enumeration, I have spoken as if the microdontal condition had been the primary one, whereas all the available evidence leads to show that the contrary was the case. The characters of all the early crania, Neanderthal Engis, and Cromagnon, are those of macrodonts. The progress has been from the macrodont to the microdont, as it probably was from the microcephalic to the macrocephalic.

The effects of the variations in size of the teeth are numerous and far-reaching. The fluctuation in the weight of the jaw depending on these variations has an important influence on the centre of gravity of the head, and affects the set of the skull on the vertebral column. This leads to a consequent change in the axes of the occipital condyles, and it is one of the factors which determines the size of the neck-muscles, and therefore the degree of prominence of the nuchal crests and mastoid process.

As the teeth and alveolar arches constitute a part of the apparatus for articulate speech, so these varieties in dental development are not without considerable influence on the nature of the sound produced. The necessarily larger alveolar arch of the macrodont is hypseloid or elliptical, more especially when it has to be supported on a narrow frontal region, and this is associated with a more extensive and flatter palatine surface. This, in turn, alters the shape of the mouth cavity, and is associated with a wide flat tongue, whose shape participates in the change of form of the cavity of which it is the floor. The musculature of the tongue varies with its shape and its motions, upon which articular speech depends, become correspondingly modified. For example, the production of the sharp sibilant S requires the approximation of the raised flexible edge of the tongue to the inner margins of the teeth behind the canines, and to the palatine margin close behind the roots of the canine and lateral incisor teeth. This closes the vocal tube laterally, and leaves a small lacuna about 5 mm. wide anterially, through which the vibrating current of air is forced. A narrow strip of the palate behind the medial halves of the median incisors bounds this lacuna above, and the slightly concave raised tongue-tip limits it below.

With the macrodont alveolar arch, and the correspondingly modified tongue, sibilation is a difficult feat to accomplish, and hence the sibilant sounds are practically unknown in all the Australian dialects.

It is worthy of note that the five sets of muscular fibres, whose function it is to close laterally the flask-like air-space between the tongue and the palate, are much less distinct and smaller in the tongues of the Australians which I have examined than in the tongues of ordinary Europeans.

There is a wide field open to the anatomical anthropologist in this investigation of the physical basis of dialect. It is one which requires minute and careful work, but it will repay any student who can obtain the material, and who takes time and opportunity to follow it out. The anatomical side of phonology is yet an imperfectly known subject, if one may judge by the crudeness of the descriptions of the mechanism of the several sounds to be found even in the most recent text-books. As a preliminary step in this direction we are in urgent need of an appropriate nomenclature and an accurate description of the muscular fibres of the tongue. The importance of such a work can be estimated when we remember that there is not one of the 260 possible consonantal sounds known to the phonologist which is not capable of expression in terms of lingual, labial, and palatine musculature.

The acquisition of articulate speech became possible to man only when his alveolar arch and palatine area became shortened and widened, and when his tongue, by its accommodation to the modified mouth, became shorter and more horizontally flattened, and the higher refinements of pronuncia-

tion depend for their production upon more extensive modifications in the same directions.

I can only allude now very briefly to the effects of the third set of factors, the sizes of the sense organs, on the conformation of the skull. We have already noted that the shape and the size of the orbital opening depend on the jaw as much as on the eye. A careful set of measurements has convinced me that the relative or absolute capacity of the orbital cavity is of very little significance as a characteristic of race. The microseme Australian orbit and the megaseme Kanaka are practically of the same capacity, and the eyeballs of the two Australians that I have had the opportunity of examining are a little larger than those of the average of mesoseme Englishmen.

The nasal fossæ are more variable in size than the orbits, but the superficial area of their lining and their capacity are harder to measure, and bear no constant proportion to the size of their apertures, because it is impossible without destroying the skull to shut off the large air sinuses from the nasal fossæ proper for purposes of measurement. Thus the most leptorhine of races, the Esquimaux, with an average nasal index of 437 has a nasal capacity of 55 c.c.m., equal to that of the platyrhine Australian, whose average is 54·5, and both exceed the capacity of the leptorhine English, which average about 50 c.c.m. There is an intimate and easily proved connection between dental size and the extent of the nasal floor and of the pyriform aperture.

These are but a few of the points which a scientific craniometry should take into consideration. There are many others to which I cannot now refer, but which will naturally occur to the thoughtful anatomist.

In this rapid review of the physical side of our subject the study of these race-characters naturally suggests the vexed question as to the hereditary transmission of acquired peculiarities. This is too large a controversy for us now to engage in, but in the special instances before us there are grounds for the presmption that these characters of microdontism and megacephaly have been acquired at some stage in the ancestral history of humanity, and that they are respectively correlated, with diminution of use in the one case, and increase of activity in the other. It is a matter of observation that these qualities have become hereditary, and the point at issue is not the fact, but the mechanism, of the transmission. We know that use or disuse affects the development of structure in the individual, and it is hard to believe that the persistent disuse of a part through successive generations does not exercise a cumulative influence on its ultimate condition.

There is a statement in reference to one of these characters which has gained an entrance into the text-books, to the effect that the human alveolar arch is shortening, and that the last molar tooth is being crowded out of existence. I have examined 400 crania of men of the long, and round-barrow races, Romano-British and early Saxon, and have not found among all these a single instance of absence of the third molar or of overcrowded teeth. On the other hand, out of 200 ancient Egyptian skulls, 9 per cent. showed displacement or disease, and $1\frac{1}{2}$ per cent. show the want of one molar tooth. Out of 200 modern English skulls there was no third molar tooth in 1 per cent. So far this seems to confirm the current opinion.

Yet the whole history of the organism bears testimony to the marvellous persistence of parts in spite of contumely and disuse. Take, for example, the present position of the little toe in man. We know not the condition of this digit in prehistoric man, and have but little information as to its state among savage tribes at the present day, but we do know that in civilised peoples, whose feet are from infancy subjected to conditions of restraint, it is an imperfect organ—

Of every function shorn
Except to act as basis for a corn.

In 1 per cent. of adults the second and third joints have ankylosed, in 3 per cent. the joint between them is rudimentary, with scarcely a trace of a cavity, in 20 per cent. of feet the organ has lost one or more of its normal complement of muscles. But though shorn of some of its elements, and with others as mere shreds, the toe persists, and he would be a bold prophet who would venture to forecast how many generations of booted ancestry would suffice to eliminate it from the organization of the normal man.

Nevertheless, although it is difficult to demonstrate, in the present imperfect state of knowledge, the method whereby race-characters have originated, I think that the most of our anthropologists at least covertly adopt the philosophy of the ancient proverb, "The fathers have eaten sour grapes and the children's teeth are set on edge."

But there are other branches of anthropology of far greater interest than these simple problems upon which we have tarried so long. The study of man's intellectual nature is equally a part of our subject, and the outcomes of that nature are to be traced in the tripartite record of human progress which we call the history of culture. It is ours to trace the progress of man's inventions, and their fruits in language and the arts, the direct products of the human mind. It is also ours to follow the history of man's discovery of those secrets of nature to the unfolding of which we give the name of science. The task is also ours to inquire into that largest and most important of all sections of the history of culture which deals with the relation of human life to the unseen world, and to disentangle out of the complex network of religion, mythology, and ritual those elements which are real truths, either discovered by the exercise of man's reason, or learned by him in ways whereof science takes no account, from those adventitious and invented products of human fear and fancy which obscure the view of the central realities. In this country it matters less that our time forbids us to wander in these fascinating fields wherein the anthropologist loves to linger, as the munificent benefaction of Lord Gifford has ensured that there shall he an annual fourfold presentation of the subject before the students of our Scottish universities. There is no fear that interest in these questions will flag for want of diversity in the method of treatment or of varieties in the standpoints of the successive Gifford lecturers.

From the ground of our present knowledge we can but faintly forecast the future of Anthropology, when its range is extended by further research, and when it is purged of fancies, false analogies, and imperfect observations. It may be that there is in store for us a clearer view of the past history of man, of the place and time of his first appearance, of his primitive character, and of his progress. But has this knowledge, interesting as it may be for its own sake, any bearing on the future of mankind? Hitherto growth in knowledge has not been accompanied with a commensurate increase in the sum of human happiness, but this is probably due to the imperfection which characterises even our most advanced attainments. For example, while the medical and sanitary sciences, by their progress, are diminishing the dangers which beset humanity, they have also been the means of preserving and permitting the perpetuation of the weaklings of the race, which, had natural selection exercised its unhindered sway, would have been crushed out of existence in the struggle for life.

It is, however, of the essence of true scientific knowledge, when perfected, that it enables us to predict, and if we ever rise to the possession of a true appreciation of the influences which have affected mankind in the past, we should endeavour to learn how to direct these influences in the future that they shall work for the progress of the race. With such a knowledge we shall be able to advance in that practical branch of Anthropology, the science of education; and so to guide and foster the physical, intellectual, and moral growth of the individual that he will be enabled to exercise all his powers in the best possible directions. And lastly, we shall make progress in that kindred department, Sociology, the study of which does for the community what the science of education does for the individual. Is it a dream that the future has in store for us such an Anthropological Utopia?

PHYSICS AT THE BRITISH ASSOCIATION.

THE mathematicians and physicists of the British Association could not have had better accommodation than that which was placed at their disposal in Edinburgh. The physics lecture-room of a University, with its appropriate fittings and appliances, is their ideal environment. Almost all the leading British physicists were present, the chief absentee of note being Lord Rayleigh, and foreign men of science were well represented by such men as Profs. von Helmholz, Wiedemann, Ostwald, and Du Bois, from Germany; M. Guillaume, from France; Schoute, from Holland; and Michelson, from America.

The *Discussion on a National Physical Laboratory* was one of the most important.

The speakers were Oliver Lodge, Glazebrook, von Helmholz, Lord Kelvin, Rücker, Dr. John Ince, Fitzgerald, Stokes, Carey Foster, Ayrton, and the President.

Prof. Lodge opened the discussion by giving an outline of the work which might be done in such a laboratory. The work should include the accurate determination of physical constants, the maintenance of standards, and the issue or verification of certified copies, the continuous recording of certain special phenomena, the conduction of certain special experimental inquiries, more particularly such as might have to be carried on years or even centuries; the taking up and completing of lines of research already developed by amateurs (or even in well-equipped laboratories) to that point at which it was impossible for them, unaided, to proceed farther.

Mr. Glazebrook described the work done at the Cavendish Laboratory. A part of this work consists in the testing of units of electrical resistance and electromotive force. In many cases it is quite impossible (in view of the more proper work of the laboratory) to give the necessary time for the proper carrying out of this work—which should be undertaken by a National Laboratory.

Prof. von Helmholz stated that one of the chief causes for the setting up of the National Physical Laboratory at Berlin was the desire of the Government that mechanicians should be assisted in their work by means of properly conducted scientific research and superintendence. It was necessary also that proper headquarters should exist for the construction and control of standards. The directors are entirely free from any duties of teaching, no systematic instruction being given in the laboratory. A large part of the work done consists in standardizing of thermometers. In the first year of the institution 90,000 were tested. Electrical apparatus, steam engine indicators, and standard lights for gas and electric companies, are also tested; and a considerable amount of thermodynamical work is undertaken with a view to practical improvements. £2500 are spent annually upon apparatus and work alone, exclusive of salaries.

Lord Kelvin said that it was a matter of great importance to the nation that its artizans should have as good scientific direction for their work as the artizans of Germany have for theirs.

Prof. Rücker said that it was by no means gratifying that it was necessary to send thermometers to Paris in order that they should be compared with the air thermometer. It would be of great advantage to have a national institution worked largely in connection with the Royal Society.

Prof. Fitzgerald, among other things, said that he doubted if the House of Commons was sufficiently educated to understand that the advance of scientific work was of national value.

The Discussion on Nomenclature of Units was opened by Prof. Oliver Lodge on lines recently indicated in NATURE.

Dr. Hopkinson and Dr. Preece criticized the proposed changes, maintaining that the time had not yet arrived when they could be advantageously introduced, even if they were satisfactory, which was very questionable except in one or two cases.

Dr. du Bois also spoke in opposition to the proposed changes, remarking that, even if accepted in Britain, they certainly would not be favoured in Germany. The discussion then dropped.

The Report on Underground Temperature dealt with observations made in a boring at Wheeling, West Virginia. The well had been sunk by a company to a depth of 4500 feet. The company decided to abandon it at this stage; but on request the boring was continued to a much greater depth for the purpose of the scientific observations. During last summer observations of temperature were made at successive depths of 125 feet down to the bottom. The surface temperature being 51°, at a little more than 1000 feet below the surface, 68°·75 were registered. At 3000 feet and 4000 feet respectively, 87° and 102° were observed; and at the bottom of the well the temperature was 110°·15. The rate of increase grows with the depth. Between 1590 feet and 1835 feet, the average rate was 1° per 92 feet; between 1835 feet and 2486 feet it was 1° per 84·5 feet. This increased until at the foot the rate was 1° per 58 feet. The average rate was 1° per 72 feet.

Report on the Discharge of Electricity from Points.—As the result of the experiments made it was found that disturbing influences, which had little or no effect at the cathode, had a powerful effect when applied to the anode, so as even to prevent the passage of sparks. Experiments were also made with the view of determining the quantity of gas concerned in the passage of a given quantity of electricity.

Report on Electrical Standards.—The committee which submitted this report had a meeting at Edinburgh, which was attended by a number of foreigners. As a result of this meeting they agreed to the following resolutions:—(1) That the resistance of a specified column of mercury be adopted as the practical unit of resistance; (2) That 14·4521 grammes of mercury in the form of a column of uniform cross-section, 106·3 cm. in height, at 0° C., be the specified column; (3) That standards in mercury, or solid metal having the same resistance as this column, be made and deposited as standards of resistance for industrial purposes; (4) That such standards be periodically compared with each other, and also that their values be redetermined at intervals in terms of that of a freshly set up mercury column. It was further agreed that these resolutions be communicated to the Electrical Standards Committee of the Board of Trade. It was agreed that the number 0·001118 should be adopted as the number of grammes of silver deposited per second from a neutral solution of nitrate of silver by a current of one ampere, and that the electromotive force of a Clark cell at 15°C. should be taken as 1·434 volts.

Prof. von Helmholz remarked that a column of mercury was much preferable to alloys, in which small fissures might exist or come into existence. He alluded also to the manner in which the difficulties of setting up such a mercury column, arising from the want of proper contact between the mercury and the glass, may be overcome. The British and German tests agreed so closely as to show that the results might be used for commercial purposes, possibly for centuries; though, for scientific purposes, some change might be needed. He and others had been sent here by their Government with the object of coming to an agreement on this subject with Great Britain, and it was hoped that America and France would also adopt the resolutions.

Wire Standards of Electric Resistance, by Dr. Lindeck, of Berlin.—The author described experiments on this subject. Alloys containing manganese seem to be the best for the purpose. Those containing zinc are the most objectionable because of impurities. Changes of resistance depending on the process of winding the coils were also investigated. The best results were got with the alloy manganin. Changes of resistance, apparently due to oxidation from contact with the air, take place; but these can be avoided by varnishing the wire. The resistance rises slowly with temperature, reaches a maximum, and then decreases rapidly.

Prof. Sylvanus Thomson said that, in working with manganin, he had found that it could not be relied upon if too strong a current were sent through it. He agreed that no alloy containing zinc should be used.

On the Clark Cell, by Dr. Kahle, Berlin.—Dr. Kahle gave details of experiments made on Clark cells. He found that they furnished a very trustworthy standard of electromotive force, and that they were very suitable for practical work.

Prof. Carhart said that he had found cells, made by different persons at different times, gave practically the same result when used under the same conditions.

Mr. Glazebrook said that he had come to the conclusion that differences amongst the results given by different cells were due to the fact that the time taken to reach the equilibrium condition differed in different cells.

Preliminary Account of Oceanic Circulation, based on the Challenger Observations, by Dr. A. Buchan.—In communicating this account, Dr. Buchan remarked that the enquiry had so far advanced that the chief results could be stated. The *Challenger* observations have been supplemented by those of Mohn, Agassiz, J. T. Buchanan, Belknapp, and Capt. Wharton. The surface winds of the globe have a special bearing on the subject of ocean temperature. The surface winds of the Atlantic generate currents which have the effect of raising the temperature on the west side of the Atlantic, at depths from 100 to 500 fathoms, about 10 degrees above the temperature at these depths on the east side. At 500 fathoms the temperature is nearly the same at both sides of the Atlantic, but at lower depths the effects are reversed. At these depths the west side is more under the influence of the Arctic currents along the American coast, and the east side is more under the influence of the under currents from the Mediterranean and the equatorial regions of the Atlantic. This high temperature distribution extends northwards even beyond the Wyville Thomson ridge between Shetland and Iceland. At 700 fathoms the temperature just south of this ridge is five or six degrees higher than it is over the Pacific, Indian, and

South Atlantic Oceans at that depth. At 200 fathoms the temperature of the Mediterranean is about 56°, and is practically constant down to the bottom (1500 fathoms in some places). Similar conditions hold in the Gulf of Mexico, where the temperature at 700 fathoms is 25°·5, with no change at lower depths. On the other hand, north of the Wyville Thomson ridge in the North Atlantic, there is a uniform temperature of about 29°·5 at all depths below 700 fathoms—that temperature being about two or three degrees higher than the freezing point of the water. This undercurrent of warm salt water from the Mediterranean, extending even beyond the North Cape of Norway, seems to explain why there is no instance of an iceberg appearing off the west coast of Europe.

Physical Condition of the Waters of the English Channel, by Mr. H. V. Dickson.—The constitution of the samples of water agreed, on the whole, with that of the *Challenger* samples, coinciding entirely with that of the Atlantic water. The tidal currents are sufficiently strong to keep the water thoroughly mixed from the top to the bottom, except off Start Bay, where a vortex is formed—the water being colder in summer, and warmer in winter, than the surrounding water, and this spot is one of the best fishing grounds in the Channel.

On Primary and Secondary Cells in which the Electrolyte is a Gas, by Prof. Schuster.—When an electric discharge passes through a part of the gas filling a tube, all the gas is brought into a state in which it readily conducts electricity. Prof. Schuster has studied the laws of this conduction. If we assume that the primary phenomena of discharge depend on dissociation of the molecules, he remarked that it must often have appeared peculiar to experimenters that no phenomena of polarization appear. When an elementary gas is used no such phenomena appear. When compound gases are used only slight polarization appears; but Prof. Schuster has found that the phenomena become very marked when hydrocarbons are used. The law of decrease of polarization in this case resembles that which is observed when liquid electrolytes are used. This points to the performance of work of the nature of electrolysis. The magnitude of the effect depends on the nature of the electrodes. It is small when copper and iron are used; but is very large when aluminium or magnesium are used. When a latter metal was employed, a direct current being passed for a long time, a reverse electromotive force of 35 volts was got from a single cell. This shows that the action is similar to that of a secondary cell. Prof. Reinold has already described cases in which gases act as an electrolyte in a primary circuit when under the influence of a discharge. Prof. Schuster has found that, in such cases, the employment of aluminium electrodes gives very strong effects.

On Leaky Magnetic Circuits, by Dr. du Bois.—It appears from the experiments described that the leakage decreases when the magnetization is increased.

Experiments on the Electric Resistance of Metallic Powders, by Dr. Dawson Turner.—It is well known that metallic powders have very great electric resistance. This can be reduced to an extraordinary degree by the passage of an electric spark in their neighbourhood. Amongst other substances Dr. Turner has tried powdered aluminium, copper, annealed selenium, iron filings, small shot, mixtures of aluminium and resin fused into a solid mass, etc. The best results were obtained with the first two. A short glass tube, filled with powdered aluminium, is placed in circuit with one or two cells and a galvanometer. No current passes until a spark discharge occurs in the neighbourhood, when a fairly large effect becomes visible. The powder continues to conduct for a short time unless it be shaken or disturbed, when the effect ceases. In the case of the rod of aluminium and resin, mere shaking does not destroy the effect, though the application of heat does. When the resistance has once been lowered in this way, the powder becomes very sensitive, a spark at a great distance produces the effect, and a very slight jar destroys it.

On the Stability of Periodic Motions, by Lord Kelvin.—The mathematical investigation of this subject was illustrated by an experiment in which a simple harmonic vertical motion was given to the point of support of a pendulum. When the period of the superposed motion was one half of that of the natural motion of the pendulum, the equilibrium became unstable, and the slightest disturbance caused the vertical motion of the bob to be changed into transverse motion of increasing amplitude. If the superposed period were now lessened, the vertical motion again became stable. Similarly a rod poised vertically in unstable equilibrium could become stable by having its point of support moved with simple harmonic motion, of proper period, in a vertical line.

Prof. Osborne Reynolds remarked that it was well known to practical engineers that a revolving shaft, when driven at a certain speed, began to bend, and might even break, though at higher speeds it would again become straight. Lord Kelvin had now explained this effect.

On the Specific Conductivity of Thin Films, by Profs. Reinold and Rücker.—When the film was an aqueous soap solution containing a considerable portion of glycerine and a small proportion of a metallic salt, the specific conductivity was the same, whether the liquid was in mass or was drawn out into a film not exceeding 1-200,000 in. in thickness. When the liquid consisted of an aqueous soap solution alone, the specific conductivity increased when the thickness became small, until, in the thinnest film observed, it was seven times as great as at first. The effect seemed to be due to a breaking down of equilibrium when the tenuity was extreme.

A Contribution to the Theory of the perfect Influence Machines, by J. Gray, B.Sc.—The theory of the perfect influence machine has been shown by Clerk Maxwell to be analogous to Carnot's theory of the perfect heat-engine. Maxwell points out that there is a loss of energy in the ordinary influence machine through sparking at the contacts, which would render the machine inefficient, even though losses from leakage and the like were done away with. Maxwell has described a machine in which sparking and the loss due to it is eliminated. This is done by causing the carrier of electricity always to make contact with charging and discharging conductors when the former is at the same potential as the latter. In the case of the discharging conductor, this is done by prolonging the contact springs to meet the carrier; in the case of the charging conductors this is not sufficient; it is necessary to surround the ends of their contact springs by two additional conductors charged to an equal and opposite potential, and of such capacity as will just reduce to zero the potential of the small quantity of electricity left from the previous discharge. These additional conductors were called by Maxwell regenerators, as being analogous to regenerators in the heat-engine.

The object of the author is to investigate the efficiency of an influence machine constructed according to Maxwell's design, and other designs less perfect. This is done by drawing a QV (quantity-potential) energy diagram for one revolution of the carrier. The results obtained are as follows:—

	Theoretical efficiency.
Maxwell's machine	1
" " without regenerators	$\frac{Q}{Q+\frac{1}{2}av}$
" " without long contacts on receivers	$\frac{2}{3}$
" " without regenerators and without long contacts on chargers	$\frac{Q}{Q+\frac{1}{2}aV}$

where Q = quantity received or discharged by a carrier in each revolution,
V = the potential (numerical value) of the positive or negative receiver,
v = the potential of the residual charge to be reduced to zero by the regenerator,
a = the part of the carrier's capacity due to its not being completely surrounded by the discharging conductor.

The conclusion is that the regenerators are of much less importance than the long contacts in adding to the efficiency of an influence machine.

Experiments with a Ruhmkorff Coil, by Messrs. Magnus Maclean and A. Galt.—The quantity of electricity induced in a secondary circuit by a make in a primary circuit is equal to the quantity induced in the same secondary by a break in the primary. If, however, there is a non-metallic gap in the circuit, the *break* impulse causes a flow in one direction, and the *make* causes either no flow, or a much less flow in the opposite direction; because the short intense impulse of the former breaks down the resistance, while the comparatively long and less intense impulse of the make either does not break down the resistance at all or only does so to a slight extent, so that the effective resistance is much greater in one direction than in the

other. To obtain the average difference of the quantity of electricity set in motion in one direction over that in the other, an electrolytic cell and a vacuum tube were placed in the secondary circuit of a small Ruhmkorff coil. The solution in the cell was sulphate of copper of density 1·7 with ½ per cent. of commercial sulphuric acid added. The mean of seven experiments, lasting from two to four hours, gave the average electrolytic current, calculated from the gain of the cathode, as ¼ of a milliampere. A similar experiment, in which the vacuum tube was replaced by a very large liquid resistance, led to no result.

The Application of Interference Methods to Spectroscopic Measurement, by Prof. A. Michelson.—Prof. Michelson's "wave-compiler" consists essentially of a small plane sheet of glass with parallel surfaces and two mirrors. The mirrors are set at right angles to each other and the central plane of the glass passes through their line of intersection, making an angle of 45° with each of them. Rays of light from the source under examination fall upon the glass surface at an angle of 45°, and are partly reflected, partly transmitted, so as to suffer normal reflection at the mirrors, and finally proceed from the other surface of the glass, at an angle of 45°, to the eye. Normal displacement of one of the mirrors parallel to itself causes a difference of path which produces interference. If the source of light emits radiation of two wave-lengths (as in the case of incandescent sodium), or of more, the brightness of the interference bands—regarded from the centre outwards—exhibits periodic variation, which can be accurately observed. The law of variation can be calculated when the distribution of light in the source, as regards wave-length and intensity, is known. Conversely, the method can be used to determine the nature of this distribution. Mr. Michelson has examined various sources of light—oxygen, hydrogen, zinc, cadmium, mercury, &c.—and has found that lines which in the most powerful spectroscope appear single, are really double, triple, or even more complex. In examining hydrogen at different pressures and temperatures the results indicated that the widths of the component lines decreased as the pressure decreased, but not without limit. Investigations made on a large number of substances give strong confirmation of the kinetic theory of gases.

On a Periodic Effect which the Size of Bubbles has on their Speed of Ascent in Vertical Tubes containing Liquid, by Dr. F. T. Trouton.—The chief peculiarity observed when a bubble of air ascends in water is that the speed of ascent is a periodic function of the size of a bubble. The form of the curve obtained by plotting the volumes of the bubbles as abscissæ and the corresponding speeds as ordinates showed that at first, as would be expected, increase in size diminishes the speed; but afterwards the speed increases in value, then reaches a maximum at about twice the minimum speed; and so on two or three times, depending on the diameter of the tube employed. The oscillations in the curve die out in much the same fashion as those of a pendulum in a viscous medium. The form of the bubbles was almost spherical at the first minimum; after this the bubble is pointed at the top until the second minimum is reached, when it is again rounded at the top, but has a dumb-bell shape, and so on, presenting in this way similarities to the breaking up of a liquid column through surface tension. Liquids which do not mix with water were used instead of air; and air bubbles in other liquids were also used.

On a Method of Determining Thermal Conductivities, by Mr. C. H. Lees.—The method has more direct application to the determination of the conductivity of a liquid than previous methods have. It consists in measuring the amount of heat conducted, under given conditions, through a film of liquid placed between two copper cylinders. It was found most convenient to keep the upper cylinder at the temperature of the surrounding air, while the lower one was kept cool by water from the mains. Precautions were taken against errors from radiation, &c.

A Magnetic Curve Tracer, by Prof. Ewing.—The apparatus is designed to plot mechanically the ordinary magnetization curve. The curve is traced on a screen by a spot of light reflected from a mirror, which is subjected to two motions—one proportional to the magnetizing force, the other to the magnetization. These motions are communicated by means of the sagging of wires placed in air-gaps in magnetic circuits. In the one case, the wire carries a steady current in a varying field; in the other the wire carries the varying current in a steady field. The curves may be traced on sensitized paper; and the instrument should be of much use to engineers for testing purposes. In working with the instrument Prof. Ewing has observed true effects of magnetic time-lag in the inward penetration of magnetization.

On a Magnetic Balance, and its Practical Use, by Prof. du Bois.—A test bar of standard size is placed within a magnetizing coil. Over this is placed an iron yoke, balanced on a knife edge, and having attached to it a graduated scale with sliding weights. When a current passes through the coil the equilibrium is disturbed, and it is restored by sliding the weight along the scale. The position of the weight then gives the magnetization in absolute measures in c.g.s. units. The position of the yoke is one of unstable magnetic and mechanical equilibrium.

On Earth Current Storms in 1892, by Mr. W. H. Preece.—In communicating this paper, Mr. Preece spoke of the great importance of observers at all parts of the globe contriving to collect data regarding electric storms.

On the Dielectric of Condensers, by Mr. W. H. Preece.—The author pointed out that in the condensers used by him there was evidence of work done upon the insulating material, which necessarily retarded the rate of propagation of signals.

On Polarizing Gratings, by Prof. du Bois.—The author has constructed minute gratings with silver wire scarcely visible to the naked eye. Radiant heat and long light waves are polarized by these gratings in the same way as electromagnetic radiations are polarized by larger wire gratings.

The Volume Effects of Magnetism, by Dr. C. G. Knott.—The results for iron tubes have been already described in NATURE. In one case a steel tube, of given bore and thickness of wall, gave increase of internal volume in all fields used. Usually the volume diminishes in low fields and increases in high. The effects were shown to the audience by projection upon a screen.

An Estimate of the Rate of Propagation of Magnetization in Iron, by Prof. Fitzgerald.—Assuming that the iron is constituted of a system of little magnets, and with possible assumptions as to the size of these magnets and their strength, it is found that their natural rate of vibration may be one hundred millions per second. Unless the period of the vibration propagated through the iron approximates to this the wave lengths would be very small; while quicker vibrations, with periods like those of light, would not be propagated at all.

Experimental Proof that the Coefficient of Absorption is not affected by Density of Illumination, by Dr. W. Peddie.—When parallel rays of light pass through a uniform absorbing medium, the intensity of the light diminishes according to a certain law. The assumption which, on this point, is made the basis of the theory of radiation is that the fractional dimunition of intensity, at any stage, per unit of thickness traversed (called the coefficient of absorption), is independent of the intensity of the light. Sir G. Stokes has indicated a method of testing the point by the reflection of light, at nearly perpendicular incidence, from the surface of glass—part of the absorbing medium being placed in the path of half of the light before reflection, and a similar part being placed in the path of the other half after reflection. Both portions, being then projected on a screen, could be directly compared in respect of colour and intensity. No test seems to have been made by this or any other method. In Dr. Peddie's method light is passed through two double image prisms and a plate of quartz. Four rays are thus produced, coloured alike in pairs, the colour of one pair being complementary to that of the other. The colour of one pair can be made to match as nearly as possible the colour most readily absorbed by the medium, similar portions of which are placed at different distances from the points from which these rays are made to diverge by means of a lens. The light being projected on a screen, a direct comparison is obtained. The media used were pieces of surface-coloured glass. In no case was any difference observed, although the intensity varied from 1 to 1000, and the eye could have observed the difference of one per cent. in the brightness of the two discs thrown on the screen, without the additional help of change of colour.

On Dispersion in Double Refraction due to Electric Stress, by Dr. John Kerr.—The fact of dispersion has been established, and it is found that the optical effect depends upon the wave-length, being the inverse ratio of the square root of the wave-length.

On a Delicate Calorimeter, by Messrs. J. A. Harker and P. J. Hartog. This is essentially a Bunsen ice-calorimeter, with solid acetic acid instead of ice, so being much more delicate, and capable of being used at ordinary temperatures.

On Graphic Solutions of Dynamical Problems, by Lord Kelvin.—The method of drawing meridional curves of capillary sur-

faces of revolution, described in "Popular Lectures and Addresses," vol. i., 2nd edition, pp. 31-42, suggests a corresponding method for the solution of dynamical problems.

Reduction of every problem of Two Freedoms in Conservative Dynamics, to the drawing of Geodetic Lines on a Surface of given Specific Curvature, by Lord Kelvin.

1. Any conservative case of two-freedom motion is proved to be reducible to a corresponding case of the motion of a material point in a plane.

2. In plane conservative dynamics, with any given value for the energy-constant, E, the resultant velocity, q, at any point (x, y) is a known function of (x, y), being given by the equation

$$q^2 = 2(E - V) \quad . \quad . \quad . \quad . \quad (1)$$

where V denotes the potential at (x, y); and every problem depends on drawing lines for which $\int q ds$ (the Maupertius "action") is a minimum.

3. Considering any part, S, of the infinite plane, find a surface, S′, such that any infinitesimal triangle A′B′C′ drawn on it has its sides q/q_0 of those of a corresponding triangle ABC in the field, S, of our plane problem; q_0 denoting the value of q at any particular point (x_0, y_0) in the plane. By the principle of least action we see instantly that the lines on S′ corresponding to paths on S, are geodetic. Thus the *adynamic* case of motion of a particle on S′, is found as a perfect and complete representative of the motion on the plane surface S, under force with any arbitrarily given function V, for its potential, and any particular given value, E, for the total energy of the moving particle.

4. It is easily proved that the surface S′, to be found according to §3, exists; and that its specific curvature (Gauss's name for the product of its two principal curvatures) at any point; is equal to

$$\frac{q^2{}_0 \, \Delta^2 \log q.}{q^2}$$

5. Examples are given of the finding of S′. As one example, illustrating the practical usefulness of this method in dynamics, the problem of the parabolic motion of an unresisted projectile is reduced to the drawing of geodetic lines on a certain figure of revolution of which the explicit equation is expressed in terms of elliptic functions.

THE PERIODIC VARIATIONS OF ALPINE GLACIERS.

THIS twelfth Report, dealing with the Alpine glaciers and their changes, by Dr. Forel, comes in the nick of time, and will be generally welcomed, for it announces that the question of glacier-changes in the Alps will in future be studied systematically; and, further, we learn in a postscript that the State Council of the Canton of Le Valais have, on the proposition of M. Maurice de la Pierre, head of the Home Department, decided to take under its efficient direction the studies of observation and control of the variations of glaciers. These observations are confided to the charge of the Cantonal Administration of Forests, the head of which is M. Antoine de Torrenté at Sion. M. Forel records this act of intelligent and prudent administration with keen satisfaction and true gratitude, and would gladly see it imitated by other cantons possessing glaciers. M. Forel publishes also the report, which, in compliance with the wishes of M. de la Pierre, he had addressed to the Home Department of Le Valais. It is equally applicable to the glaciers of other cantons in Switzerland, and we therefore print it *in extenso.*

M. de la Pierre, Councillor of State, head of the Home Department, Sion.

SIR,—Referring to the interview you granted me on January 31, and in reply to your question, I have the honour to give you the following particulars:—

Glaciers in general, and particularly those of Le Valais, are subject to variations in shape, which, according to an irregular periodicity, cause them sometimes to grow in length, in breadth, in thickness, sometimes to decrease, often in very considerable proportions. These variations, which in recent centuries have attracted the attention of the populations interested and of naturalists, have in this century been the subject of direct study, especially during the last twenty years.

It has been recognized that most of the great catastrophes which have ravaged the region of the high Alps, have been caused by these glacial variations. It is when the glacier extends, lengthens, arrives at its maximum, that it not only invades the fields and destroys Alpine chalets, but barricades the valleys, arrests the flow of rivers, and creates temporary lakes, the evacuation of which ravages the country; or else, surpassing its usual dimensions, it forms an avalanche, the destructive power of which is terrible. Taking my examples from Le Valais, I attribute to forces of this kind: the catastrophes of the Valley of Saas, 1633, 1680, 1772, caused by the overflow of the lake Mattmarck, due to the stoppage of the Viège by the glacier of Allalin; the catastrophes of the valley of Bagne, 1545, 1605, 1818, caused by the formation of a temporary lake behind the barricades of the glacier of Giétroz; the catastrophes of Randa, 1636, 1819, caused by the fall of the glacier of Bies, which had assumed extraordinary dimensions; perhaps we might also attribute to the same source the inundations of St. Bartholomew, 1560, 1635, 1636, 1835, which may have been due to the excessive increase and fall of the terminal extremity of the glacier of Plan-névé of La Dent-du-Midi.

Since these variations of glaciers are the cause of great catastrophes in mountainous regions, they are deserving of attentive study; there is scope to form theories and to recognize the rhythm of their periodicity; it is very necessary to be able to foresee their development, in order to ward off threatening events.

Now, the preparatory study which we have made within the last few years has shown us that the periodicity of glacial variations is much longer than was formerly believed to be the case; the popular dictum that the increase in the size of glaciers recurs every seven years is certainly incorrect. We cannot yet give definite figures, but probably the cycle of glacial variation is as much as 35 to 50 years. The latter period alone has been studied attentively; if 1850 or 1855 be fixed upon as the epoch of maximum of glaciers, they have been steadily decreasing in past years, so that from 1870 to 1875 we were not aware of a single one on the increase. In 1875 the Glacier des Bossons du Mont Blanc gave the signal for a new period by commencing to lengthen out; it was followed in 1878 and 1879 by the glaciers of Trient and Zigiorenove, then successively by some thirty glaciers in different valleys of Le Valais; but the phase of increase is not yet general in your canton; a number of large glaciers, Arolla, Otemma, Corbassière, Le Gorner, Le Rhone, are still decreasing or stationary. It is only of the Mont Blanc group that the increase can be said to be general; in Le Valais it is in process of development, and we are still very far from the maximum stage of glaciers. If, as is probable, the maximum only arrives at the commencement of next century, the actual period of glaciers will have lasted more than fifty years.

Thus this is a phenomenon, whose cycle is equal or superior to that of the average human life; one generation of men witnesses only one of the glacial variations. It is a phenomenon of such majestic slowness that its study is exceptionally difficult.

A phenomenon of which a man can, in his whole life, see but one manifestation, surpasses in its amplitude the powers of initiative individual study. To observe the facts of so prolonged a period, organizations are required of a superior duration. Shall we address ourselves, therefore, to learned societies, which, being continually renewed, may be supposed to have sufficient continuance? We fear that, even in the most powerful of these societies, Societies of Naturalists and the Alpine Clubs, a sufficiently keen interest in observations, which can only be utilized after the lapse of some generations will not be felt for the observations to be organized and carried out with the necessary perseverance. It seems to us that the State alone, by virtue of the indefinite continuance it enjoys, is in a position to follow out this study with a sufficiently long grasp. However much we may be in favour of private individual or collective initiative action in researches and scientific work, in this special case we believe it advisable to have recourse to the State administration, considering it the only institution of sufficient duration to proceed to the study of phenomena of such extreme slowness.

We therefore take the liberty of addressing ourselves very respectfully to the Government of the Canton of Le Valais, and begging it to introduce the study and observation of the variations of glaciers, which have so great an influence on the prosperity of mountain populations.

It seems to us that the State department best fitted for such a

study would be that of the Administration of Forests. The forest inspectors and their agents are called upon by their functions to travel over the high valleys of the Alps; they would thus be able, without great additional labour, to undertake the observation of glaciers.

As to the programme on which they should work, we would simplify it as much as possible, and reduce it to two points:—

1. To attentively survey the glaciers in order to fix for each one the year of maximum and the year of minimum extent in their successive variations.

2. To specially watch the dangerous glaciers, and warn the Administration of the danger they may cause by assuming exaggerated dimensions during their phase of development.

In order to carry out this double programme, the Government should charge each inspector of forests to study the glaciers of his district, and to inscribe on a register *ad hoc* the state of growth and decline of each glacier every year. For important glaciers, interesting or dangerous, he should have measurements made from fixed marks, and state in exact figures the changes in the dimensions of the glaciers; for little, uninteresting, or unimportant glaciers, occasional inspection and the reports of the mountaineers would suffice to ascertain their state of growth or decrease.

M. Antoine de Torrent, Inspector-General of the Forests of Le Valais, has long been occupied in collecting observations on the glacial variations of our Alps; it is from him that we have obtained all the facts recorded by science in this field of research in Le Valais; it is for him, not for us, to give instructions as to the way in which the observations should be organized.

This study is not an expensive one; it calls for no great outlay on the part of the State, nor does it make great demands on the powers and the time of the observers. It may lead to important and useful results. It can only be successfully carried out by the State, since that alone has the necessary persistence to continue it long enough. I therefore venture to recommend these studies on the variation of glaciers to your great benevolence, and to the enlightened solicitude with which you follow all questions interesting to the public welfare. Men of science, who consecrate their lives to the study of the phenomena of nature, are ready to help you to the best of their ability to study these questions from a theoretic point of view. But it is necessary, in order to arrive at practical results, to have a collection of materials of observation which the State alone seems to us capable of bringing together with success.

Accept, M. le Conseilleur de Etat, the expression of my very respectful and devoted consideration.

F. A. FOREL.

Morges, February 10, 1892.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 8.—M. de Lacaze-Duthiers in the chair.—The "Pythonomorphs of France," by M. Albert Gaudry. Announcing the discovery of the snout of one of the great chalk reptiles termed pythonomorphs by M. Cope, on account of their similarity to the sea serpent as imagined by the ancients. The specimen is from a pythonomorph 10m. long, and was found in the upper chalk of Cardesse, near Pau. It is similar to the *Mosasaurus giganteus* of Maestricht, and has been termed *Liodon mosasauroides*. A smaller and somewhat similar specimen was also found, and was termed *Liodon compressidens*. These and a few minor fragments are the first representatives of the pythonomorphs found in France.—On the production of sugar in the blood at the expense of the peptones, by M. R. Lépine.—On the lava of July 12, 1892, in the torrents of Bionnasay and Bon-Nant (catastrophe of Saint-Gervais, Haute Savoie), by M. P. Demontzey. After describing the probable course of the catastrophe, the writer comes to the following general conclusion:—That the lava of July 12 has behaved exactly like those which have been observed before in the torrents of the Alps and the Pyrenees. That its energy was all the more disastrous as the transport in masses commenced in the most elevated regions of the torrent basin after the sudden bursting forth of a large body of water concentrated more rapidly even than in the most violent hailstorms in the upper basins of torrents without glaciers. That the volume of the deposited materials of all sorts—estimated at about one million cubic metres—presents no anomaly in comparison with the relatively small amount of water, which effected the transport by a series of successive bounds, with alternate momentary accelerations and retardations of speed. That this torrent phenomenon has substituted for a simple and hitherto harmless rivulet a torrent whose activity can be mastered with a relatively short delay. That both in the Alps and the Pyrenees similar cases of the transformation of peaceful rivulets into formidable torrents can be cited, aggravated by the fact of their being caused by rain, which is even more difficult to predict and ward off than the dangers presented by a glacier. And lastly, that this great disaster could not have been provided against, since nobody had had the idea even of exploring the glacier of Tête-Rousse.—On a property of lamellar bimetallic conductors submitted to electromagnetic induction, by MM. Ch. Reignier and Gabriel Parrot. An arrangement recalling Faraday's disc is obtained by substituting for the ordinary copper conductors thin plates composed, along their thickness, of a very magnetic and a highly conducting metal, so placed that the lines of force are perpendicular to their thickness. The flow of induction emanating from the north pole is divided into several sheets of parallel lines very close together, which only traverse the magnetic portions of the bimetallic conductors, and the tubes of force become cylindrical. The available energy in such an arrangement increases at a rate which is sensibly proportioned to the height of the conductors. An apparatus constructed on this principle gave, with a weight of 750 kg. and a velocity of 500 revolutions, 32,000 watts giving an output of 42 watts per kg. of the machine.—The application of the measurement of density to the determination of the atomic weight of oxygen, by M. A. Leduc. The composition of water by volume, and thence its composition by weight, were determined by finding the density of a mixture of hydrogen and oxygen produced by the electrolysis of an alkaline solution. After an electrolysis of several days, during which the superfluous gas was allowed to escape through mercury, the liquid and the platinum poles were saturated with gas, and the density obtained by the method previously described did not vary by more than 0·0001 gr. The value within $\frac{1}{100}$ per cent. was 0·41423. The volume ratio between hydrogen and oxygen was 2·0037 at 0°, and the atomic volume of oxygen 1·9963. The atomic weight of oxygen by this method is 15·877, and by the synthetic method 15·882, so that 15·88 must be taken for the mean atomic weight. Hence the molecular weight of water vapour is 17·88, and its theoretical density 0·622.—On the general form of boiling-point curves for central substitution compounds, by M. G. Hinrichs.—Note on the existence in the earth of an acid mineral substance as yet undetermined, by M. Paul de Mondesir. If all the carbonic acid contained in lime be driven off by a strong acid, and the ratio of lime to carbonic acid be carefully measured, the lime is found to exceed the quantity necessary for saturation. The earth remains always acid and capable of decomposing carbonate of lime in the cold. That this acid residuum cannot be humic acid or free silica is proved by the total destruction of the organic substances by ignition or potassium permanganate, which leaves the property in question unaffected. The quantity of acid matter varies from ·2 to 1 per cent. of the earth. It is very stable, and its composition has not yet been determined.—Calcareous soap and boiler explosions, by M. A. Vivien.—Pupine, a new animal substance, by M. A. B. Griffiths. This is extracted from the skin of the chrysalis of several lepidoptera.—On the colouring matter of *Micrococcus prodigiosus*, by the same.—On the coccoid state of a nostoc, by M. C. Sauvageau.—On an alga living in the roots of the *Cycadeæ*, by M. P. Hariot.—On the presence of fossils in the azoic formations of Bretagne, by M. Charles Barrois.—On the discovery of cut flints in the quaternary *Rhinoceros Mercki* alluvium of the Saône valley at Villefranche, by M. Ch. Depéret.

ROME.

R. Accademia dei Lincei, June 5.—The 289th annual meeting, honoured by the presence of H. M. King Umberto I.—The President introduced the two committees charged with the examination of the works in competition for the two royal prizes of 10,000 lire each, one for social and economic sciences, the other for mathematical sciences. Senator Lampertico, reporting for the first committee, said that, although two essays, one on "Ancient Socialism," by Salvatore Cognetti de Martiis, and another on the laws of the distribution of wealth, bearing the motto, "Laboremus," had shown considerable merit, the committee had not felt justified in awarding the prize to either.

Professor Cerruti, on behalf of the second committee, reported that two candidates had been found equally deserving of the mathematical prize, viz., Professor Luigi Bianchi by his essays on the triple orthogonal systems of Weingarten and allied subjects, and Professor Salvatore Pincherle in virtue of his various works on the general theory of functions. It was therefore decided to divide the prize equally among these two candidates. —The last of the ministerial prizes for professors of secondary classical and technical schools, amounting to a total of 9000 lire for philological, and 9000 lire for physical and chemical subjects, were distributed among thirteen candidates, prizes of 3000 lire being obtained by Professors Nasini and Costa for their work "On the Variation of Refractive and Dispersive Power of Sulphur in its Compounds," and by Dr. Enrico Salvioni for his contribution "On the Construction of the Legal Ohm."—General Ferrero then addressed the meeting on the subject of scientific measuring instruments. Although, he said, the human eye, that natural model of the telescope, the microscope, and the photographic camera, can distinguish within a few hundredths of a millimetre whether two points are in contact; although the ear can appreciate sounds ranging from 32 to 70,000 vibrations per second, and is able, while following the rhythm of a full orchestra, to discover the slightest dissonance; yet the power of our senses is limited to a comparatively small portion of the infinite variety of external phenomena, that portion which is of more immediate value for our merely animal life. The errors which the unaided senses are liable to lead us into are mainly due to their subjectivity, which renders the impressions of one individual incomparable with those of another, or with his own under different conditions. The use of instruments enables us to submit these impressions to measurement, to compare them amongst themselves, and immensely to extend our field of investigation towards the infinitely great and the infinitely small. The progress made in this direction during the last few hundred years justifies the hope that the time is not far distant when the results of observation will be as far as possible beyond the personal influence of the observer. The disciple of science will read the truth in the book of nature, traced out by the phenomena themselves. The universe, which has always remained inaccessible to metaphysics, will willingly disclose its secrets to the researches of modern science. This owes its great progress during the last century mainly to the perfection and delicacy of its measuring instruments, which has made modern astronomical observations a thousand times more accurate than those of the Chaldees, and has, by making very minute differences of temperature appreciable and measurable, enabled biology to enter the ranks of the exact sciences. The accuracy of measurements of length and mass is ensured by the arrangements in connection with the International Office of Weights and Measures at Bréteuil. Some recent comparisons of standards gave a probable error in length of 1/20,000mm., while that for mass was 4/1000 mgr. The determination of weight has been carried to such a pitch of accuracy, that it has been found possible, at Bréteuil, at Monaco, and at Rome, to measure the slight differences of weight produced by varying the height above the ground by a few metres. For the measurement of time there has been no necessity of fixing a conventional standard. The marvellous invention of the pendulum has made it possible to subdivide almost indefinitely the natural fundamental unit, the duration of the rotation of the earth. In the determination of longitudes the error has been reduced to one or two hundredths of a second. Hipp's chronoscope, which may be called a microscope for time, enables the observer to subdivide time to a thousandth of a second. The impulse given to biological research by such instruments has been astonishing. The time of reaction to the various sensory stimuli has been fixed at 136 thousandths of a second for sound, at 150 for light, 133 for touch, 359 for taste, and 443 for smell, while the velocity of propagation of a nervous impulse has shown itself to be 37m. per second. In artillery, the chronoscope has been utilized for the study of the initial velocities of projectiles, and for the tracing of diagrams expressing the relations between spaces, times, and explosive pressures. Errors of observation may be due to the imperfection of the senses, to unavoidable faults in the construction of the instruments, and to external influences. These may be classified according as they are constant or accidental, or better as periodical or otherwise. Most of the errors due to the observer, and of those due to external influences, are periodical, and may be eliminated by repeating the observations under varying conditions. The calculus of probabilities shows that the precision of results, so far as the elimination of purely accidental errors is concerned, increases with the square root of the number of observations. But experience shows that beyond a certain number of observations the increase of precision is illusory. This is probably due to the existence of other errors of a constant character which escape analysis, and from which it is not possible to protect the observations. Experiment also proves that for all kinds of work the maximum error does not exceed a certain limit, which is a function of the mean error. For angular measurements, this does not exceed three times the mean error, so that according to Gauss's law of errors it would be safe to lay 1000 to 1 against the chance of an error greater than 3·2 times the mean error. The observer himself must above all have physical qualities enabling him to use his senses under the best possible conditions. In addition to well-trained senses and facility in managing his instrument, he must have a clear mind, a correct judgment, and a sound scientific preparation for the research he undertakes. Concentrated upon his research, he must abstract himself from the surroundings among which he lives, and possess a spirit unimpassioned enough to subject himself to a purely objective criticism. In concluding, the speaker pointed out that there is at present no science which treats of measurement in general, as a preparation to all the sciences which aim at quantitative results. Many treatises on astronomy, on geodesy, on physics are prefaced by theories of instruments and the compensation of errors. But even those works which profess to treat of the art of measuring are usually limited to geodetic and topographic measurements. It is to be hoped that this important vacancy may soon be filled up, and that a *Science of Measurement* will unite the elements dispersed among the various sciences in one compact and harmonious whole.

CONTENTS.